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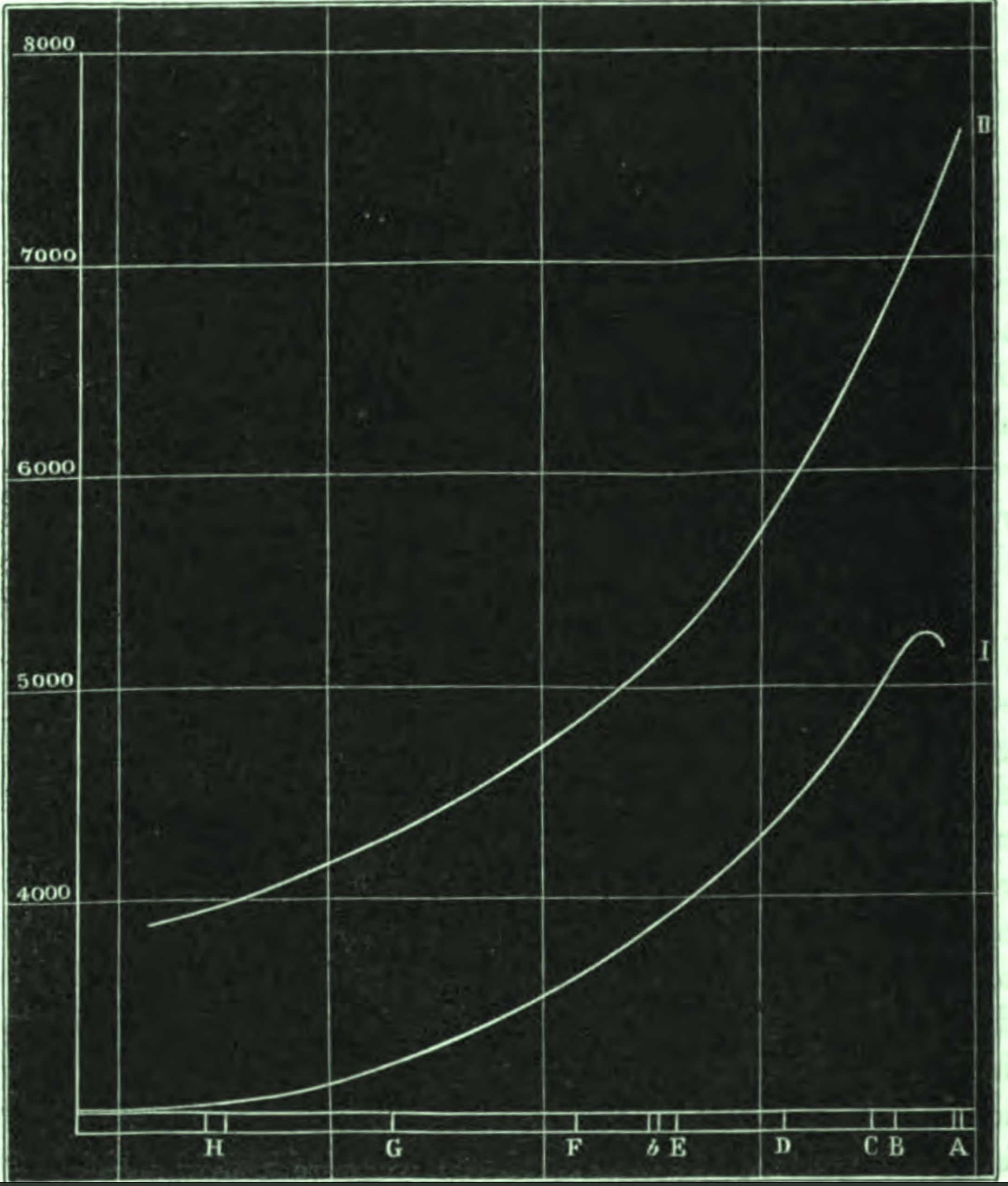

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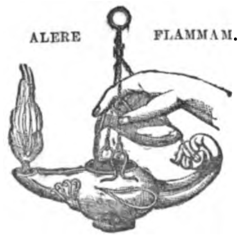

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PROCEEDINGS

OF

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JANUARY 1879.

Starch and Unannealed Glass under the Polariscope.

By WALTER BAILY*.

[Plates I.–IV.]

THERE are many bodies, of which a spherical grain of starch and a circular plate of unannealed glass may be taken as specimens, having an optical structure symmetrical about an axis through the body. The object of this paper is to investigate the state of the light which emerges from such a body, when monochromatic light in any state of polarization is sent through the body in the direction of the axis.

In fig. 1 (Plate I.) let S S′ and T T′ be drawn through R perpendicular to one another, and let U U′ and V V′ bisect the angles between them. Suppose a quarter-undulation plate to be fixed parallel to the paper, with its axes parallel to S S′ and T T′—and the light to be passed perpendicularly to the paper through a Nicol's prism having its axis perpendicular to the paper and its plane of polarization inclined at an angle ρ to the line S S′, then through the quarter-undulation plate, and then through the body, which is also to be placed with its axis perpendicular to the paper.

Let the paper represent a section of the light after it has emerged from the body. Take any point P and draw round it an ellipse representing the polarization of the light at P.

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* Read June 28, 1878.

The state of the light will be completely determined if we know the angle (α) which the axis major of this ellipse makes with R P, the angle (β) which a line joining the extremities of the axes of the ellipse makes with the axis major, and the direction in which the rotation takes place.

Let the angle $SRP=\phi$; produce R P to X and draw $PY \perp PX$; take any point Q on the ellipse, let $PQ=r$, and the angle $QPX=\theta$.

The resolved part of the vibration at P along R P has been retarded in passing through the body differently to the resolved part perpendicular to R P. Let the resolved part along R P have been retarded by a quantity σ more than the mean amount, and the other part have been retarded by the same quantity less than the mean amount. The quantity σ is a function of the distance R P.

Let $\sin t$ represent the vibration in the æther after the light has passed through the Nicol. This is equivalent to

$$\cos\rho \sin t \qquad \parallel SS'$$

and

$$\sin\rho \sin t \qquad \parallel TT'.$$

After passing through the quarter-undulation plate the vibrations become

$$\cos\rho \sin(t+45^\circ) \qquad \parallel SS'$$

and

$$\sin\rho \sin(t-45^\circ) \qquad \parallel TT'.$$

These may be written (the coefficient $\frac{1}{\sqrt{2}}$ being omitted)

$$\cos\rho (\sin t+\cos t) \qquad \parallel SS'$$

and

$$\sin\rho (\sin t-\cos t) \qquad \parallel TT'.$$

It is easy to show that these vibrations represent motion in an ellipse whose axes are parallel to SS′ and TT′, and the extremities of whose axes are joined by a line making an angle ρ with the axis major,

These vibrations are equivalent to

$$\cos(\rho-\phi)\sin t+\cos(\rho+\phi)\cos t \qquad \parallel PX$$

and

$$\sin(\rho-\phi)\sin t-\sin(\rho+\phi)\cos t \qquad \parallel PY.$$

After passing through the body the displacement $\parallel$ PX is $r\cos\theta$, and that $\parallel$ PY is $r\sin\theta$. Hence

$$r\cos\theta = \cos(\rho-\phi)\sin(t+\sigma) + \cos(\rho+\phi)\cos(t+\sigma),$$
$$r\sin\theta = \sin(\rho-\phi)\sin(t-\sigma) - \sin(\rho+\phi)\cos(t-\sigma);$$

which may be written

$$r\cos\theta = a\sin t + b\cos t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$
$$r\sin\theta = a'\sin t + b'\cos t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$\begin{aligned} a &= \cos(\rho-\phi)\cos\sigma - \cos(\rho+\phi)\sin\sigma, \\ b &= \cos(\rho-\phi)\sin\sigma + \cos(\rho+\phi)\cos\sigma, \\ a' &= \sin(\rho-\phi)\cos\sigma - \sin(\rho+\phi)\sin\sigma, \\ b' &= -\sin(\rho-\phi)\sin\sigma - \sin(\rho+\phi)\cos\sigma. \end{aligned}$$

Differentiating (1) and (2) with respect to t, we have

$$\cos\theta\frac{dr}{dt} - \sin\theta r\frac{d\theta}{dt} = a\cos t - b\sin t, \quad . \quad . \quad . \quad (3)$$

$$\sin\theta\frac{dr}{dt} + \cos\theta r\frac{d\theta}{dt} = a'\cos t - b'\sin t. \quad . \quad . \quad (4)$$

Multiplying (3) by $r\sin\theta$, and (4) by $r\cos\theta$, and subtracting, we have

$$\begin{aligned} r^2\frac{d\theta}{dt} &= (a\sin t + b\cos t)(a'\cos t - b'\sin t) \\ &\quad -(a'\sin t + b'\cos t)(a\cos t - b\sin t); \end{aligned}$$

whence

$$\begin{aligned} r^2\frac{d\theta}{dt} &= a'b - ab' \\ &= \sin 2\rho\cos 2\sigma - \cos 2\rho\sin 2\sigma\sin 2\phi. \quad . \quad (5) \end{aligned}$$

Again, in (1), (2), (3), (4), let t have a value which makes r a maximum or minimum; then θ becomes α, and $\frac{dr}{dt}$ vanishes.

Eliminating r from (1) and (2), we get

$$(a\sin\alpha - a'\cos\alpha)\sin t = -(b\sin\alpha - b'\cos\alpha)\cos t;$$

and eliminating $r\frac{d\theta}{dt}$ from (3) and (4), we get

$$(b\cos\alpha + b'\sin\alpha)\sin t = (a\cos\alpha + a'\sin\alpha)\cos t.$$

Hence

$$\frac{a\sin\alpha - a'\cos\alpha}{b\cos\alpha + b'\sin\alpha} + \frac{b\sin\alpha - b'\cos\alpha}{a\cos\alpha + a'\sin\alpha} = 0,$$

or

$$2(aa' + bb') - (a^2 + b^2 - a'^2 - b'^2)\tan 2\alpha = 0;$$

that is,

$$\sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi + \cos 2\rho \cos 2\phi \tan 2\alpha = 0. \quad (6)$$

Now $r^2 \frac{d\theta}{dt}$ is proportional to the area of the ellipse, as the period of vibration is constant; and the axes of the ellipse are proportional to $\sin \beta$ and $\cos \beta$, since the intensity of the light, and consequently the sum of the squares of the axes is constant over the whole section Hence the area of the ellipse is also proportional to $\sin \beta . \cos \beta$ (that is, to $\sin 2\beta$). Putting $\rho = 45°$ and $\sigma = 0$ in (5), we get $r^2 \frac{d\theta}{dt} = 1$. But in this case the emerging light is circularly polarized, and consequently $\beta = 45°$, whence $\sin 2\beta$ also equals unity. Hence in every case

$$r^2 \frac{d\theta}{dt} = \sin 2\beta.$$

Hence

$$\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi - \sin 2\beta = 0. \quad . \quad (7)$$

The locus of points at which the major axis of the ellipse of polarization is inclined at a constant angle to the radius RP will be called an "isoclinal line," and will be denoted by the symbol $K(\alpha)$, where α is this angle. $K(\alpha)$ and $K(\alpha - 90°)$ are both included in the equation (6), as that equation does not distinguish between major and minor axes.

The locus of points at which the line joining the extremities of the axes makes a constant angle with the major axis will be called an "isomorphal line," and will be denoted by the symbol $M(\beta)$, where β is this angle. Equation (7) is the equation to $M(\beta)$.

The direction of rotation of the æther is positive or negative according as $\sin 2\beta$ is positive or negative—that is, according as β is positive or negative, since we need only give β values lying between $\pm 45°$.

In figs. 2, 3, 4, 5 these loci are represented—the isomorphals by continuous lines, and the isoclinals by dotted lines. The Arabic numerals indicate the values of α in degrees, and the Roman numerals those of β in degrees.

The value given to σ is $RP + 90°$, a function which has been assumed solely for convenience in drawing the figures. The figures are drawn between the limits $\sigma = 180°$ and $\sigma = 360°$.

An extension beyond these limits would give merely a repetition of the portion within them, since an addition of 180° to the value of σ makes no difference in the equations to the loci. Directions from the centres of these figures will be referred to by means of the lines in fig. 1.

The values of ρ are in fig. 2, 45°, in fig. 3, 30°, in fig. 4, 15°, and in fig. 5, zero. Accordingly the incident light is circularly polarized in fig. 2, elliptically in figs. 3 and 4 (the eccentricity of the ellipse being less in fig. 3 than in fig. 4), and plane-polarized in fig. 5.

The isomorphals and isoclinals drawn are those for which the values of α and β are 45°, 30°, 15°, and zero. The thick continuous lines are branches of M(0), which is the locus of plane-polarized light. The round spots are loci of circularly polarized light, or M(±45).

The symbol $K(\pm\alpha)$ will be used to include $K(\alpha)$, $K(\alpha-90°)$, $K(-\alpha)$, and $K(90°-\alpha)$; its equation is

$$(\sin 2\rho \sin 2\sigma + \cos 2\rho \cos 2\sigma \sin 2\phi)^2 = \cos^2 2\rho \cos^2 2\phi \tan^2 2\alpha. \quad (8)$$

And the symbol $M(\pm\beta)$ will be used to include $M(\beta)$ and $M(-\beta)$; its equation is

$$(\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi)^2 = \sin^2 2\beta. \quad . \quad (9)$$

Putting $90°-\phi$, or $-90°-\phi$, for ϕ in these equations makes no change in the equations. Hence $K(\pm\alpha)$ and $M(\pm\beta)$ are each symmetrical with respect to UU′ and VV′.

Adding (8) and (9), we get

$$\cos^2 2\phi . \cos^2 2\rho = \cos^2 2\alpha . \cos^2 2\beta, \quad . \quad . \quad . \quad (10)$$

from which equation it appears that the intersections of $K(\pm\alpha)$ with $M(\pm\beta)$ all lie on the two diameters defined by the equation; and since α and β are interchangeable in the equation, we see that $K(\pm\gamma)$ and $M(\pm\delta)$ intersect on the same two diameters as $K(\pm\delta)$ and $M(\pm\gamma)$.

Let

$$\sigma = \left(\frac{n}{2} + \frac{1}{4}\right)\pi + \vartheta;$$

then $K(\pm\alpha)$ becomes

$$(\sin 2\rho \cos 2\vartheta - \cos 2\rho \sin 2\vartheta \sin 2\phi)^2 = \cos^2 2\phi \cos^2 2\rho \tan^2 2\alpha,$$

and $M(\pm\beta)$ becomes

$$(\sin 2\rho \sin 2\vartheta + \cos 2\rho \cos 2\vartheta \sin 2\phi)^2 = \sin^2 2\beta.$$

From these equations it appears that, in changing the sign of ϕ, we shall change only the sign and not the magnitude of ϑ; so that if we have drawn part of $K(\pm\alpha)$ or $M(\pm\alpha)$ between the limits $\phi=0$ and $\phi=45^\circ$, we can draw the corresponding part of the curve between the limits $\phi=0$ and $\phi=-45^\circ$. If σ' be the value of σ corresponding to $-\phi$,

$$\sigma'=\left(\frac{n}{2}+\frac{1}{4}\right)\pi-\vartheta,$$

and therefore

$$\sigma+\sigma'=\left(n+\frac{1}{2}\right)\pi. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Putting $\alpha=0$ and $\beta=0$ in (8) and (9), we get for $K(0)$,

$$\sin 2\rho \sin 2\sigma+\cos 2\rho \cos 2\sigma \sin 2\phi=0\,; \quad . \quad . \quad (12)$$

and for $M(0)$,

$$\sin 2\rho \cos 2\sigma-\cos 2\rho \sin 2\sigma \sin 2\phi=0. \quad . \quad . \quad (13)$$

Let s be the value of σ where a radius ϕ intersects a branch of $M(0)$, and let $s+\xi$ be the value of σ where the same radius intersects a branch of $K(\pm\alpha)$; then by (8) we have

$$\begin{aligned}\{(\sin 2\rho \sin 2s+\cos 2\rho \cos 2s \sin 2\phi)\cos 2\xi \\ +(\sin 2\rho \cos 2s-\cos 2\rho \sin 2s \sin 2\phi)\sin 2\xi)\}^2 \\ =\cos^2 2\rho \cos^2 2\phi \tan^2 2\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)\end{aligned}$$

But by (13) the coefficient of $\sin 2\xi=0$, and ξ is determined by $\cos 2\xi$ only, from which it appears that ξ has pairs of values of equal magnitude and opposite sign. Hence, if we have drawn a part of $K(\pm\alpha)$ on one side of a branch of $M(0)$, we can draw the corresponding part on the other side of $M(0)$.

A similar property of $M(\pm\beta)$ with respect to $K(0)$ may be proved in the same manner.

Where $K(\pm\alpha)$ intersects $M(0)$, ξ vanishes and the diameter becomes a tangent. Its position is determined by putting $\beta=0$ in (10). This gives

$$\cos^2 2\phi \cos^2 2\rho=\cos^2 2\alpha. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

The position of the diameter tangents to $M(\pm\beta)$ is determined by putting $\alpha=0$ in the same equation. We get

$$\cos^2 2\phi \cos^2 2\rho=\cos^2 2\beta. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Hence the same diameters are tangents to $K(\pm\gamma)$, $M(\pm\gamma)$ at the points where they intersect $M(0)$ and $K(0)$ respectively.

If we put $\sigma\pm45^\circ$ for σ in K(0) we obtain M(0), and conversely. See equations (12) and (13). . . . . . (17)

If we put $\sigma\pm90^\circ$ for σ in $K(\pm\alpha)$ or $M(\pm\beta)$, the equation is unaltered; and by this means from one branch of one of these curves we can obtain all other branches of the curve. (18)

We will now consider the forms of the loci, dealing first with figs. 3 and 4, in which the incident light is elliptically polarized.

To obtain the isomorphals, we have the equation (7) for $M(\beta)$ and (13) for M(0).

Putting $\beta=\rho$, we obtain the following equation to $M(\rho)$, viz.

$$\sin\sigma(\sin 2\rho\sin\sigma+\cos 2\rho\cos\sigma\sin 2\phi)=0\,; \quad . \quad (19)$$

and putting $\beta=-\rho$, we obtain the following equation to $M(-\rho)$, viz.

$$\cos\sigma\,(\sin 2\rho\cos\sigma-\cos 2\rho\sin\sigma\sin 2\phi)=0. \quad . \quad (20)$$

$M(\rho)$ therefore consists of circles for which $\sin\sigma=0$, and ovals intersecting those circles on the diameters SS′ and TT′; and $M(-\rho)$ consists of circles for which $\cos\sigma=0$, and ovals intersecting those circles also on SS′ and TT′. The outer circles in the figures are circles of $M(\rho)$; and the middle circle is one of the circles of $M(-\rho)$.

When $\beta=\pm45^\circ$, the light becomes circularly polarized, and therefore the value of α becomes indefinite; consequently in equation (6) we must have $\cos 2\phi=0$. Putting this value of $\cos 2\phi$ in (7), we get the following values of ϕ, σ, and β at points where the polarization is circular:—

$$\left.\begin{array}{ccc}
\phi. & \sigma. & \beta. \\
+45^\circ, & n\pi+\ 45^\circ+\rho, & -45^\circ \\
+45^\circ, & n\pi+135^\circ+\rho, & +45^\circ \\
-45^\circ, & n\pi+\ 45^\circ-\rho, & +45^\circ \\
-45^\circ, & n\pi+135^\circ-\rho, & -45^\circ.
\end{array}\right\} \quad . \quad (21)$$

These points will be called the "circular points." All the isoclinals pass through all the circular points. The sign of β indicates the direction of rotation of the æther. Where the sign is positive, the direction of rotation has not been altered by the passage of the light through the body; where the sign is negative, the direction of rotation has been reversed.

To draw the isomorphals:—

Mark the circular points by (21). Draw the circles of $M(\rho)$ and $M(-\rho)$ by (19) and (20). Draw one branch of $M(0)$ from $\phi=0$ to $\phi=45^\circ$ by (13).

Obtain a branch of $K(0)$ by (17).

Obtain an oval of $M(\rho)$ between the above limits by (14).

Draw the part of a branch of $M(\beta)$, for example, M(XV.) in fig. 3, or M(XXX.) in fig. 4, which lies on one side of $M(0)$, and complete on the other side by (14).

Draw the remaining branches of $M(\pm\beta)$ between the above limits by (18).

Draw the isomorphals between $\phi=0$ and $\phi=-45^\circ$ by (11), and complete the figure by means of the symmetry about UU′ and VV′.

Write against the isomorphals the values of β, taking care to make the sign of β the same as that at the nearest circular point.

To draw the isoclinals:—

Equations (6) and (8) are not in a form available for calculation; but by solving (8) as a quadratic in $\sin 2\phi$, we obtain the equation to $K(\pm\alpha)$ in the form

$$\sin 2\rho \cos 2\sigma \sin 2\sigma \cos^2 2\alpha + \cos 2\rho(1-\sin^2 2\sigma \cos^2 2\alpha)\sin 2\phi$$
$$= \pm \sin 2\alpha (\cos^2 2\rho - \sin^2 2\sigma \cos^2 2a)^{\frac{1}{2}}, \quad . \quad (22)$$

from which, by putting successive values for σ, we can obtain corresponding values of ϕ.

By putting $\alpha=\rho$, we obtain the equation to $K(\pm\rho)$, viz.

$$\sin 2\rho \cos 2\sigma - \cos 2\rho \sin 2\sigma \sin 2\phi = \pm \sin 2\phi. \quad . \quad (23)$$

We have already obtained a branch of $K(0)$. Draw from (23) the part of a branch of $K(\rho)$ which lies on one side of $M(0)$ between the limits $\phi=0$ and $\phi=45^\circ$, and from (22) obtain a similar part of a branch of $K(\pm\alpha)$—for example, K(15) in fig. 3 and K(30) in fig. 4.

Complete these branches by (14).

Complete the other branches between the same limits by (18).

Draw the figure between $\phi=0$ and $\phi=-45^\circ$ by (11), and complete the figure by the symmetry about UU′ and VV′.

Draw straight lines in the direction UU′ and VV′ to give

K(± 45), for which the equation is

$$\sin 2\phi = 0. \qquad \ldots\ldots \qquad (24)$$

To number the isoclinals:—

Note in (6) that when $\sigma = n\pi$, $\tan 2\alpha = -\tan 2\phi$, or $\alpha = -\phi$; and when $\sigma = (n+\frac{1}{2})\pi$, $\tan 2\alpha = \tan 2\phi$, or $\alpha = +\phi$. Hence the intersections of the isoclinals with the circles of K(ρ) graduate that circle in the negative direction, and their intersections with the circle of K($-\rho$) graduate that circle in the positive direction. Graduate these circles accordingly, taking care to deduct 180° from the graduation when it exceeds 90°, and 360° from it when it exceeds 270°, and the readings will give the values of α.

The deductions are made to keep the readings low, and for the sake of symmetry.

From the figures 3 and 4 it appears that M($\pm\rho$) divides the figure into regions of two kinds: one kind, which I will call the "segments," contains all the points at which the light is more circularly polarized than the incident light; and the other kind, which I will call the "rings," contains all the points at which the light is more plane-polarized.

In the segments the isomorphals are closed curves surrounding the circular points; and in the rings the isomorphals are closed curves surrounding the centre of the figure.

It also appears that K($\pm\rho$) divides the figure into regions of two kinds—one containing all the points at which both the axes of the ellipse are inclined to the radius by a greater angle than ρ, and the other containing all the points at which one of the axes is inclined at a less angle than ρ. Both kinds of regions are four-cornered, and have two opposite corners on circular points; but in the first kind both these circular points lie on the same radius, and in the second the circular points lie on different radii. The isoclinals in each region pass from one circular point to the other.

Comparing fig. 3 with fig. 4, we see that as ρ increases, the segments become smaller, and the isomorphals in the rings become more circular. When $\rho = 45°$, as in fig. 2, the segments vanish, and the isomorphals all become circular, the equation to M(β) becoming

$$\cos 2\sigma - \sin 2\beta = 0.$$

The circular points are retained in the figure to show its relation to the other figures; but the whole circles through them are loci of circularly polarized light.

The equation to K($\pm\alpha$) gives $\tan 2\alpha=\infty$;

$$\therefore\ \alpha=\pm 45^\circ.$$

The equation to K($\pm\rho$) becomes

$$\cos 2\sigma=\pm\sin 2\phi.$$

This curve is retained in the figure to show the continuity with the other figures—although, as the inclination is everywhere 45° or −45°, the points on the curve have no special properties. For the same reason the straight lines K(± 45) are retained.

Again, comparing figs. 3 and 4, we see that as ρ diminishes, the segments increase; and at last, in fig. 5, where ρ vanishes, the segments fill the whole space and the rings vanish. Each closed curve of M(0) is forced into a broken line consisting of quadrants of circles joined by pieces of diameters; and as these closed curves now touch at their angles, they form together a complete system of circles and diameters whose equation is, putting $\rho=0$ in (13),

$$\sin 2\sigma \sin 2\phi=0.$$

The isoclinal K(0) also forms a system of circles and diameters; their equation is, from (12),

$$\cos 2\sigma \sin 2\phi=0.$$

All the other isomorphals form closed curves round the circular points; their equation is, from (7),

$$\sin 2\sigma \sin 2\phi+\sin 2\beta=0.$$

And all the other isoclinals pass from one circular point to another on the same radius; their equation is, from (6),

$$\cos 2\sigma \tan 2\phi+\tan 2\alpha=0.$$

The diameters of K(0) and M(0) coincide; this is indicated in fig. 5 by the thick continuous line having dots on one side of it.

The figure may be drawn in a similar manner to that described for figs. 3 and 4.

The locus of plane-polarized light may be investigated without reference to the condition of the rest of the light, by drawing M(0) by equation (13), and marking the direction of polarization at successive points on it by equation (15).

In figs. 6, 7, and 8 the points through which short straight lines are drawn are points at which the light is plane-polarized; and the short straight lines through them show the direction of polarization at the points. The dotted lines connecting these points are loci of plane-polarized light. The centres about which small circles are drawn are points at which the light is circularly polarized; and in fig. 6 the dotted lines connecting these circles are loci of circularly polarized light. The signs within the circles indicate the direction of rotation of the æther; see (21).

In fig. 6, $\rho=45^\circ$; in fig. 7, $\rho=22^\circ\ 30'$; in fig. 8, $\rho=0$.

If the light be passed through an analyzing Nicol with its plane of polarization inclined at an angle ρ' to SS′, we can obtain the intensity of the light at any point as follows:—

The vibration along the major axis is $\cos\beta\cos t$, and that along the minor axis is $\sin\beta\sin t$; so that the vibration in the direction of the plane of polarization of the analyzer is

$$\cos(\rho'-\alpha)\cos\beta\cos t-\sin(\rho'-\alpha)\sin\beta\sin t.$$

Hence, if I is the intensity of the light after passing the analyzer,

$$I=\cos^2(\rho'-\alpha)\cos^2\beta+\sin^2(\rho'-\alpha)\sin^2\beta,$$

$$2I=1+\cos 2(\rho'-\alpha)\cos 2\beta.$$

The appearance of the light after passing an analyzer might be calculated from this equation, but can be inferred more readily by an inspection of the figures, which show its state before passing.

We notice that two dark spots will be seen on each branch of M(0), one at each extremity of a diameter, at the points where the vibration is perpendicular to the plane of the analyzer. The spots on the successive branches of M(0) will be alternately on a certain diameter and on the diameter perpendicular to it.

When the incident light is circularly polarized, these spots will move round in circles with unaltered appearance and at a uniform rate as the analyzer is turned uniformly. See figs. 2 and 6.

When the incident light is elliptically polarized, the spots will move round the curves M(0); but the rate and appearance will vary (see figs. 3, 4, and 7). For on the circles of $M(\rho)$ the major axis of the ellipse preserves a constant direction in space, since $\phi+\alpha=0$; but on the circles of $M(-\rho)$ the major axis rotates uniformly in space with an angular velocity double that of the radius, since $\phi-\alpha=0$. Hence in those portions of M(0) which are near the circles of $M(\rho)$, the change in the direction of the vibration will be slow; so that in this part the spot will be elongated, and will move more rapidly than the analyzer is rotated: but in the parts of M(0) near the circles of $M(-\rho)$, the change in the direction of the vibration will be rapid; so that in these parts the spot will be shortened, and will move more slowly than the analyzer is rotated.

When the incident light is plane-polarized (see figs. 5 and 8), the slow-changing parts of M(0) have combined to form the inner and outer circles of the figure and the diameters S S′ and T T′. Along these lines the direction of vibration has no change, but remains constantly the same as that of the incident light; but on the middle circle of the figure, and on corresponding circles, the direction of vibration rotates uniformly with a velocity double that of the radius. Hence on the latter circles there will be spots moving uniformly round with a velocity double that of the analyzer; but on the other parts of the figure there will be no spots. However, when the spots on the latter circles reach the diameters, then the former circles and the diameters will become black.

If the light is not monochromatic, these appearances will not be so distinctly seen, as the absence of one colour will not occur exactly in the same place as the absence of another, since the position of the isomorphal and isoclinal lines depends upon the value of σ, and this will differ for different colours. But the position of diameters which give plane-polarized light in figs. 5 and 8, is not dependent on the value of σ; and hence with any light this cross will always appear uncoloured, being black when the upper and lower Nicols are crossed, and in full light when they are parallel.

If σ is constant, the isomorphals and isoclinals become straight lines from the centre, and the state of the polarization may be conveniently represented by taking a series of points in a circle round the centre, and drawing about each point the ellipse of polarization at that point. The ellipse will show the polarization along the radius on which it lies. This is done in figures 9 to 13, in which σ is about 15°. In fig. 9, $\rho=45°$; in fig. 10, ρ lies between 45° and σ; in fig. 11, $\rho=\sigma$; in fig. 12, ρ lies between σ and zero; and in fig. 13, ρ is zero.

Suppose now the light to be passed through an analyzer placed with its plane of polarization in the direction T T′. When $\rho=45°$, the quadrants about U U′ will be dark, and those about V V′ will be light. They will gradually shade into one another, there being no black or full light. As ρ diminishes, U U′ becomes darker until, when $\rho=\sigma$, U U′ is black (see fig. 11). As ρ further diminishes, the black bar opens out into a dark oblique cross, neither bar of which is black; and when ρ becomes zero, this cross becomes rectangular and black. As ρ passes on to $-\sigma$, the cross becomes oblique and not black, and closes up into a black bar along V V′; and when ρ becomes $-45°$, the quadrants about V V′ are dark, and those about U U′ light. When we have the oblique cross, we can by a suitable turn of the analyzer make either arm of the cross black. (See fig. 12.)

If the analyzer is placed with its plane of polarization in the direction S S′, we get the same set of appearances, except that we get light for dark and dark for light; and in the case of the single bar and rectangular cross, we get full light instead of black.

The appearances presented when σ is variable may be well seen in cylindrical disks of unannealed glass. I do not know of any bodies which show very clearly the appearances presented when σ is constant. Crystals of salicene show the black cross remarkably well, and give indications of the single black bar; but in this substance σ, though constant along each radius, varies in passing from one radius to another, and this completely hides the phenomena of the oblique cross.

However, in grains of *tous-les-mois* starch, phenomena closely analogous to those above described as presented when σ is constant may be easily observed under a moderately high power—the only difference in the phenomena being that, in consequence of the grain of starch being generally an unsymmetrical body, the lines are distorted, the black cross, for instance, being neither rectangular nor rectilinear. See "The Optical Properties of Starch," Phil. Mag. for August 1876.

II. *An easy Method for Adjusting the Collimator of a Spectroscope. By* ARTHUR SCHUSTER, *Ph.D., F.R.A.S.*

THE ordinary method for adjusting the collimator of a spectroscope for parallel rays is only applicable to the mean rays of an achromatic combination. At the extreme ends of the spectrum a readjustment has to be made. If the ultra-violet rays are observed, and if the lenses are of quartz, the ordinary method cannot be used. The following method is so simple that I cannot help thinking it has often been in use; yet I have nowhere seen it described, and I know that others, like myself, have often found a difficulty in making the adjustment without much loss of time and with simple apparatus.

The adjustment, as the following consideration will show, can be made on each line of the spectrum without any apparatus whatever. The only requirement is that the prism should be movable.

Suppose the rays which fall on the prism to be either convergent or divergent; then, after their passage through the prism they will seem either to converge to or diverge from a point, which is the secondary focus: as the prism is turned, so as to change the first angle of incidence, the secondary focus will change. If the rays are strictly parallel, then, whatever be the position of the prism, the focus will not be altered. This, then, is a delicate test for ascertaining whether rays proceeding from the collimator are parallel or not. It remains to be shown how it can be converted into a rapid method to put the collimator into the right adjustment.

The three fundamental equations for the passage of a ray of light through a prism,

$$\sin i = n \sin r,. \qquad \ldots \ldots \ldots \quad (1)$$

$$\sin i' = n \sin r', \qquad \ldots \ldots \ldots \quad (2)$$

$$r + r' = \alpha, \qquad \ldots \ldots \ldots \ldots \quad (3)$$

give

$$\frac{di'}{di} = -\frac{\cos i \cos r'}{\cos i' \cos r}. \qquad \ldots \ldots \quad (4)$$

In these equations i and i' are the angles which the ray makes with the first and second surfaces respectively on entering and leaving the prism; r and r' the two corresponding angles of refraction, and α the angle of the prism. The right-hand side of equation (4) will, as a little reflection will show, steadily decrease when i is increased from 0 to $\frac{\pi}{2}$. This shows that the greater the first angle of incidence the more nearly parallel are the rays. The following system of consecutive approximation will therefore give the desired result.

Suppose the collimator is out of adjustment: move the telescope slightly out of position of minimum deviation; then two positions of the prism exist which will bring the desired ray into the middle of the field. Call the position in which the first angle of incidence is greatest A, the other B.

1. Put the prism into the position A, and focus the *telescope* until the line in question, either dark or bright, is distinctly seen.

2. Move the prism into position B, and focus the *collimator* until the same line is distinctly seen.

3. Repeat the operation, always focusing the telescope when the prism is in position A, and the collimator when the prism is in position B. After three or four trials no change of focus is required; both collimator and telescope will then be adjusted for parallel rays. I find that it is by no means necessary to work much out of the position of minimum deviation in order to gain a delicate adjustment. If the adjustment is made in the centre of the field, then I usually put the telescope into such a position that the line, when the prism is placed at maximum deviation, should just be out of the field

of view; this gives quite a sufficient change of focus if the rays are not parallel on entering the prism.

The following measurements, which were purposely made without special care, will show the accuracy of which the method is capable. The sliding tube of the collimator was divided into millimetres. Two different adjustments for the sodium-line, made in the way described above, gave the readings 5·0 and 4·0. The prism was now turned round so as to deflect the ray to the other side. Two adjustments now gave 4·1 and 5·0. The mean of the four readings is 4·5. The adjustment was then made according to the well-known method of first focusing the telescope on a distant object and focusing the collimator to the telescope afterwards: the reading was 4·2. As the focal length of the collimator was 300 millimetres, the two results differ only by a thousandth part of the focal length. Whether this difference is due to errors of observation, or whether it is produced by a difference in the focus of the yellow rays and the mean visible rays, I cannot say; but I believe, with a little precaution, the method can be adapted to the study of the achromatism of a lens.

I have assumed that the faces of the prism are perfectly plane. Practically it is difficult to get a prism in which this condition is accurately fulfilled; and it may be questioned whether the curvature of the prism may not seriously interfere with the accuracy of the method. To this I reply:—

1. That a prism which is known to be good may always be set aside to do this work.

2. That the reason of having the rays strictly parallel on entering the prism is based on the supposition that the faces of the prism are plane. It is by no means evident that parallel rays will give the best definition when the faces of the prism are curved.

3. That the change in the adjustment of the collimator introduced by the curvature of the prism is very small. One prism, which I know to be exceptionally bad, gave a difference of a half per cent. in the focal length of the collimator. It is not the change of focus introduced by the curvature of the prism which makes the method inaccurate when the prism is bad, but the difference in the change of focus in the two positions of the prism. This is one of the reasons why it is

better to take the two positions of the prism not too far away from minimum deviation. The small displacement of the prism will only introduce a small variation in the focal length due to the curvature of the faces.

III. *A Condenser of Variable Capacity, and a Total-Reflexion Experiment. By* C. V. Boys, *A.R.S.M., Lecturer for the Term on Natural Science at Uppingham School.*

Wishing to show my pupils the effect of condensation on the spark, I thought a condenser the capacity of which could be reduced gradually to nothing would be most suitable. So I made this simple contrivance, which answered its purpose well:—

A glass tube is sealed at one end and is covered with tinfoil for one third of its length; this forms the outer coating. The inner coating consists of a test-tube with the rim cut off, also covered with tinfoil; this is fixed to a wire, and can be drawn in and out. When it is fully in, the condenser has its maximum capacity; when drawn out as far as possible, the two coatings are too far apart to have any sensible action, and the capacity is zero.

On hanging this on the conductor of a Holtz machine the effect on the spark is well shown. Let the wire be first pushed in as far as possible, the condenser then acts to its full extent; but on gradually drawing it out the sparks are less and less bright, but follow one another more and more rapidly, till at last, when it is fully out, they have passed gradually to the almost continuous pale spark so characteristic of a Holtz machine. To show the effect best, the poles should not be more than about half an inch apart. Of course much ozone is formed inside the tube.

The total-reflexion experiment was an accident. A small condenser made of a test-tube gave way under the strain, a minute hole being pierced in the bottom, through which sparks passed almost continually. No light could be seen anywhere except on the rim of the tube, which formed a brilliant circle of light. The light from the spark was totally internally reflected in the substance of the tube till it reached the rim, which it struck normally. The bright circle of light (the tube itself being dark) was very striking; and the experiment is a far truer illustration of total internal reflexion than the more beautiful one with a stream of water. The tube is, unfortunately, broken; and I have not succeeded in piercing another with the spark. A crack made with a hot wire does not do so well.

IV. *On the Music of Colour and Visible Motion.*

By Professors John Perry *and* W. E. Ayrton*.

[Plates V. & VI.]

At the present time, when musical instruments of one form or another are employed nearly throughout the whole world, when even the emotions evoked by the sounds of the human voice have given life to the efforts of a whole nation in the 'Marseillaise,' we are apt to forget that our feelings may be excited through other media than sound. But, just as now all kinds of musical instruments are used in rendering the works of great composers, so we may expect that all known methods of exciting emotion will be combined in the grand emotional compositions of the future.

Although our feelings may be worked on through the medium of any of our senses, one only of these has been hitherto cultivated in the highest degree. And the reason of this is, that there exists an infinite number of *easy* ways of producing sound; so that combinations of sound have been used as the vehicle for exciting emotion in us, and in our forefathers, for the last four hundred years; and, as a result, the ear has been slowly trained to act as the conveyer of the varied impressions it is the province of the artist to create, whereas the means in our power of acting through the eyes are even up to the present day clumsy and inadequate.

Of the optical methods hitherto employed to work on the emotions, the oldest is certainly sculpture; but this can never create an emotion unconnected with thought; and the feelings produced by it vary much in different people, and even in the same person at different times. The musical composer, on the other hand, is able to produce a definite succession of emotions which he can vary at will, and which are not utterly different in different people. A piece of sculpture is but suggestive—it merely introduces some simple emotion which acts in a controlling way upon the human mind; so also a fine picture induces a dreamy state and sets one a thinking.

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* Abstract of a paper read November 23rd, 1878.

Other emotions, again, are excited in particular people by certain associations connected with a taste, smell, &c.; but these may be likened to the energy stored up in gunpowder or nitroglycerine, to be set free by the smallest accident, whereas the efforts of real emotional art may be likened to those of which the effects may be calculated according to known laws.

For the eye to act as an agent for the emotions, it must receive much cultivation. Even with the experience of sound the ear has gained during the last four hundred years, how very few people are there sufficiently educated to have their feelings excited through music in answer to the emotions of a composer? and how pleasing to all is the *repetition* of a strain in a melody or the movement in a dance, perhaps from the instantaneous education the ear or the eye receives which enables it to better understand the movement when repeated. Consequently, if we consider the cumbrous and expensive nature of the apparatus necessary for producing regular changes of colour, or of motion, or of the size of the moving bodies, we may expect that it will probably take a long time before the world is able to employ our more complicated agencies.

It may appear at first sight that in placing motion on a footing of equality with what *we* consider its sister graces—sculpture, painting, and music,—we offered an indignity to these latter; and it may appear inconceivable to many how any amount of study of moving bodies can ever create an art as powerful and as enchanting as music. But it must be borne in mind that our present form of the fine arts probably only owes its existence to the accident that western nations have more assiduously educated the emotional side of their minds in certain particular directions*.

And in our own country we have a close connexion between the varied emotions created by colour or movement and those excited by sound. It is well known that when certain persons hear an Oratorio, an Opera, or even a well-played violin solo, they see, without any voluntary effort on their part, beautiful changing mosaics, the patterns of which have definite connexions with the musical chords, and that such people always see a flash of light when they hear a sudden shriek of a railway-whistle.

The emotions excited by large bodies having a great velocity do not seem to be producible by any thing else in nature. These are felt when we stand on a bridge over a railway when a train approaches and passes underneath at great speed, or when we stand at the side of a railway when the train passes, even if we hear no sound, or if there is no appreciable trembling of the earth. In our first experience of such visible motion, terror, due perhaps to its utter strangeness, predominates. Similarly, the emotions produced by the play of colours in a fine sunset, by the rolling of the waves on the sea-shore, by the rhythmic motion of a large engine or of a long pendulum, by the tracing out of the combined harmonic curves, which have now become well known to all who have heard lectures on vibrating bodies, seem to be quite different from one another, and from the emotions produced by sound.

We have therefore tried to make a machine which to the emotions produced by a combination of colour, a mass in motion, and by motion in curved paths, would bear the same relationship as a musical instrument to music. We have not ventured to give our machine a name, since the name of the instrument should be that of the art; and although we have a name for exciting emotion by sound (music), we lack names both for the art of exciting emotion by colour, and by moving bodies.

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* Here followed a number of examples taken from the Japanese stage and musical performances, proving that great conventionality existed among different nations in the expression of the emotions, and lending weight to the doctrine that music, unlike painting, received no suggestion from nature, and was therefore a creation of each individual people.

Many instruments have already been devised for combining together two harmonic motions; but as the conception of using such machines as emotion-exciters has never been present in the designers' minds, the performances of these instruments, although very beautiful, are necessarily of a comparatively elementary nature. The most important of these machines, all of which, with the exception of one or two Prof. Guthrie has been so very kind as to have arranged in working order before us, are Blackburn's pendulum, Wheatstone's kaleidophone, Lissajous' tuning-forks, Yeates's vibrating prisms, Donkin's harmonograph, Tisley's and Spiller's harmonograph, and Hopkins's electric diapason. In some of these, as in Blackburn's pendulum, only *one* particular pair of harmonic vibrations can be combined, and any change in the period of either means a stoppage of the instrument, corresponding in music with a delay in the tune at the end of every chord; in others we can change the period of one or other of the component harmonic vibrations, but have no certain means of controlling the amplitude, which in music would be equivalent to an inability to render at will a note *forte* or *piano*; or, rather, as it is not only the strength of the entire note, but even the amplitude of the various component harmonics that these instruments cannot regulate, it would be as if in music there was the probability of a note marked in the score as *piano* for the flute being rendered by a loud blast on the trumpet. In only one of these harmonic-motion compounders with which we are acquainted, viz. in the most perfect of the existing ones, Tisley's harmonograph, can an elliptic and a linear motion be combined; but even in this case a change from this motion to any other can only be made by first stopping the machine; and in none of the machines can a sudden change be produced in phase. Now it is obvious that we require a motion-producer of far greater range than any of these, if we are to play on the emotions, since all the qualities—elation and depression, velocity, intensity, variety, and form (which it is considered are possessed by a complex emotion)—must be visibly rendered.

The result produced by our instrument is this:—A round shadow is thrown upon the plain white wall of the room in which the audience are seated. The shadow appears to be a large black ball, of which the size during the performance may be made to vary. It has motions over the wall which are pure harmonic or combinations of harmonic motion, produced by our being able to give to the shadow two independent motions —one in a vertical line, the other in a horizontal line, *each* consisting of a *combination* of linear harmonic motions, the *amplitude*, *period*, or *phase* of any one of which may be varied at will. We give a collection in figures (A to U, Plate V.) of some of the most simple paths traced out by our moving ball, and which differ from the ordinary Lissajous' figures in that we have placed little circles at such distances asunder as are passed over in *equal times*, in order to give an idea of how the velocity varies at different parts of any such path. Unfortunately, however, for giving a conception of the appearance, it is this variation of velocity, the effect of which on the senses no figure or description can give any idea, which constitutes one of the most striking features of the exhibition. Many of our readers will, however, have seen the motions of a very large Blackburn's pendulum, and of a very long simple pendulum; and they will therefore have gained such an idea of the motions of which we speak as a person, who has heard only the chirp of a sparrow, has about Beethoven's sonatas.

It must be remembered that while all the possible paths of the moving body are beautiful in shape, they are also endless in their variety. Not only may they vary without limit in their form, but also in their size, and in the velocity with which the body moves along them. Thus, perhaps the body is swinging slowly in a straight line in any direction, like the swaying of a huge tree in the wind, or so rapidly that a dark line only is visible, when, touching a key, the line is suddenly seen to open out, and the body rolls round a small or great circle, or a small or great ellipse of any proportions and with any velocity*; or the figure may be like any of the simple figures illustrated in the diagrams, or any others of the millions of different and more complicated forms producible. Some may change all their dimensions gradually in a certain direction, while dimensions in the other directions remain unchanged;

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* One of the most beautiful things in connexion with the recent monster captive balloon in Paris was its rolling round and round in the breeze, like a huge inverted conical pendulum, after the stay ropes had been liberated, just before its ascent.

others may alter all their dimensions in different directions simultaneously, but with different velocities. Now, as we gaze at the body with all these graceful complex motions, which can readily be varied in shape or size of path, or in velocity of description, the same sort of awe comes over us as must have been felt by the people when they first listened to the strains of the earliest musical instrument.

To be sure it can never be as easy to change from a grave circular swing to a quick and complicated motion as it is to put down the key of a pianoforte; and it may be long before mechanicians will be able to let us vary the motion with sufficient rapidity. But we found that we could, even with our imperfect instrument, change one form of motion to almost any other possible one in about a second; and this is sufficiently quick for the present educational state of the art.

Our instrument itself we unfortunately cannot show, as it is the property of the Japanese Government, and is in Japan. Some photographs, however, of it and of our assistants who took part in its construction and performances are lying on the table. The exhibitions have been confined to ourselves and our students; but it was our intention, after we had educated ourselves by practising with the machine, to exhibit it publicly. Unfortunately, however, from various delays connected with improvements and alterations necessarily connected with the designing of such a new machine, it was hardly completed before one of the present writers left Japan, and the other has now no opportunities to practise with it. But it is our intention to construct a new instrument with many improvements on the first model, one of the most important being the carrying out of the idea we had at the commencement, of enabling the operator, by touching keys, to give any desired brilliant or sombre coloration to the wall on which the shadow of the body is moving, or to play on the wall a changing mosaic.

We will now describe the simplest form of our instrument, represented in fig. 1 (Plate VI.) about one sixteenth of its full size.

B C is a roller which is turned by a handle D (not visible in the figure), the fly-wheel E being of use in steadying the motion. The roller is divided into three portions, B F, F G, G C, by the circular collars at F and G; at H, J, K, the centres of these portions, the section of the roller is circular, whereas at any other place it is such that, acting like a cam or tappet, it gives pure harmonic motion to a slider kept pressing on the roller, and only capable of radial motion. Such a sliding piece, resting any where between K and C, receives one complete harmonic motion during one revolution of the roller; if it rests anywhere between G and K, it completes two pure harmonic motions during a revolution, three between J and G, four between F and J, five between H and F, and six between B and H. The amplitude gradually decreases as the sliding piece is made to rest on places nearer the circular section, where of course there is no up and down motion of the sliding piece.

Every section, therefore, of the roller has a curved outline, of which the construction is easy. Thus suppose we want the section which will give a slider five complete harmonic swings in one revolution of the roller. Make the angle A O B (fig. 2) equal to one fifth of four right angles; describe the circular arc A B with O as centre and radius O A equal to $R+r$, where R is the radius of each of the three circular parts of the roller H, J, K, and r the radius of the small friction-wheel on the end of the sliding piece; make B D equal to half the maximum swing we wish the slider to receive. Then with centre B and radius B D describe the semicircle 159, which divide into any number of equal parts (eight in the figure), and let fall perpendiculars 22, 33, 44, &c. from the points of subdivision on to the diameter D B 9, meeting it in the numbered points. With O as centre, describe a circular arc through each of these numbered points in D B. Divide the angle A O B into twice the number of equal parts into which the semicircle 159 was divided. Then draw a curve 1 C D through the intersections of the first arc and first radius, the second arc and second radius, &c. Finally, draw a great many equal circles with radius r, the centres being in the curve; then the envelope D E F (fig. 3) is one fifth of the whole curved section we wish the curved roller to possess; and a template of tin may be made to be used in the construction of the roller. It will be found advisable to construct four templates for each division of the roller, since, although a section of the harmonic surface described by the *centre* of the little friction-wheel formed by a plane passing through the axis of the large roller in a straight line, such a section of the actual surface on which the little wheel rests will be curved in consequence of r, its radius, not being infinitely small. Our large roller was made of hard wood; but it would have been much better if it had been made of cast iron or steel, since when of wood it, as well as the little friction-wheel of the sliders, must be made large to avoid abrasion; but even when these are large it is very difficult to avoid abrasion, and consequent slight irregularities in the motions of the slider, since the springs pressing down these sliders must be moderately strong to cause them to promptly follow all the alterations in curvature of the different parts of the roller.

Fig. 5 shows (reduced to a scale of one sixth) the roller as it came from the lathe before being cut to fit the templates; and figs. 6, 7, 8, 9, 10, 11 the sections at A B, C D, E F, G H, I J, K L (fig. 5) one quarter full size. Small circles have been drawn representing the little friction-wheel, and at such distances apart that in all cases the time taken for the wheel to pass from one position to the next is constant and equal to the forty-eighth part of the period of the revolution of the roller.

We used six sliders, α, β, γ, ϵ, ζ, η (fig. 1), one of which is shown enlarged in fig. 4. Each of these sliders could be moved longitudinally, parallel to the axis of the roller, along two stout iron bars L M, O P (fig. 1), in order to alter the amplitude of the swings. Sliders α and ϵ could be made to rest anywhere between B and F, β and ζ anywhere between F and G, and γ and η anywhere between G and C. Each slider carried at its upper end a large pulley made to move very easily (fig. 4); and the three pulleys of α, β, γ were always in the plane of the fixed pulleys T, U, V attached above to the wooden frame. A fine inextensible cord passing round the movable pulleys α, β, γ, and the fixed pulleys, S, T, U, V, was fixed at one extremity to the pulley V (the purpose of which was simply to adjust the length of the cord); and at the other end, where it hung vertically, it was attached to the top of the glass plate *a b c d*, from the bottom of which hung a weight in a pail of water to damp the motion of the weight. This system then gave to the glass plate the sum of the motions of the sliders α, β, and γ. A similar cord over ϵ, ζ, η and W, X, Y, Z, together with the cord passing over the fixed pulley Q, and to which also hangs a weight in a pail of water, gives to the glass plate a horizontal motion equal to the sum of the harmonic motions of the sliders ϵ, ζ, η. It is evident, therefore, that to a circular patch stuck on the centre of the glass we were able to give motions compounded of the above harmonic motions perpendicular to one another, and by projection, by means of an electric light or heliostat, to cause this motion to appear like that of a large black ball rolling about on a white or coloured background.

Even still greater variety could have been imparted to the figures by the metal rods L M, O P (on which the sliders moved) having a motion at right angles to the radius of the roller. This could easily be arranged if the ends of the metal rods moved in circular grooves ; and the result would be that not only could we alter the amplitude of any one of the component harmonic vibrations by moving a slider along the rod, but we could also alter the phase by giving the rod a circular motion. To do this, however, satisfactorily would have required either the employment of a much larger roller, so that the slowest vibration of a slider occurred four or five times during one revolution of the roller, or else a change in the arrangement. In the illustration (fig. 1) the glass plate, for simplicity is shown merely kept in position by the four cords ; but in reality it moved in a horizontal frame which again slid in a vertical one, so that any lateral motion at right angles to the plane of the glass was impossible.

The reason why there is necessarily a considerable distance between the sets of fixed and moved pulleys is in order that a longitudinal motion of the slider shall not alter the *mean* horizontal or vertical position of the spot on the glass. At first we had the cords much longer than shown in the figure ; but then, even after great care had been taken in endeavouring to obtain inextensible cords, some stretching was found to take place in practice ; consequently we were compelled to determine experimentally what was the greatest length of cord that could be used without the stretching interfering with the accuracy of the motion ; and this length was the one employed in the actual apparatus.

The ingenious way in which a number of pulleys are made to give the sum of their motions to the extremity of a cord was suggested to us by the arrangement employed in Sir W. Thomson's tide-calculating machine; but it is possible that in our new machine we shall adopt a totally different plan, and one which we think is new. If the two extremities of a long rigid rod have parallel motions perpendicular to the rod, the middle of the rod has a motion equal to half the sum of the extremities. Thus the parallel motions of two, four, or 2^n points may be compounded. Similarly for the three points, one third of the sum of parallel motions is obtained from the centre of a rigid triangular piece of which the points are the corners; so that by bars and frames of simple construction it is easy to get the sum of the parallel motions of any number of pieces.

We think the roller-plane has much to recommend it; but a series of little cranks may be better. Let a number of parallel shafts, having their bearings near their ends on two sides of a trough-shaped metal frame, receive from a system of spur-wheels near their centres relative velocities 1, 2, 3, &c. Let A (fig. 13) be either end of any shaft, B being the corresponding bearing; and let there be attached to it, and therefore revolving with it, a sort of double wheel of metal of the form shown. I I is the pulley round which the cord passes, if it is by means of a cord that we sum the harmonic motions; or the pulley may be absent; and the axle of the pulley is then simply a crank-pin. Then the rate of revolution of the shaft A determines the period, the distance of the axle F G from the shaft the amplitude, and the position of F G relative to a fixed diameter K L the epoch of the component harmonic motion. By shifting, then, the axle F G we may alter the epoch for any multiple of $\frac{\pi}{4}$ for the greater amplitudes, and of $\frac{\pi}{2}$ for the smaller. As there is, however, only a limited change of epoch, we think, on the whole, that our improved roller method is to be preferred.

We have used combinations of pure harmonic motions for obvious reasons; but it is possible that, instead of each section of the roller giving a pure harmonic motion, it may be found more suitable to have it giving some other kind of periodic motion; and such an instrument will differ from the preceding in pretty much the same way that one musical instrument differs from another. As various means have already been devised, by revolving sheets of parti-coloured glass, for producing the effects of the chromotropes of the magic lantern, which physiologists have informed us produce such marked and instantaneous effects on the nervous constitution, and physical organs, of children, we have not yet specially turned our attention to the mechanical details of the colour portion of our machine.

In what has preceded we have spoken only of projecting the motion of a single ball on a wall; but there is no reason why the motions of several balls should not be gazed at simultaneously, nor why the people of a large city should not have an exhibition of the colour and motion art upon a canopy of clouds on a dark night.

For assistance rendered us during the construction of this apparatus, and for the general intelligent interpretation of our wishes, we have to thank our late assistant, Mr. Kawaguchi, one of the brightest of the students of the Imperial College of Engineering, and one whose constant earnestness of purpose, while it rendered his life the more valuable to the scientific development of Japan, now makes his recent death the more to be deplored.

V. *On the Determination of the Variation of the Thermal Conductivity of Metals with Temperature, by means of the permanent Curve of Temperature along a uniform thin Rod heated at one end. By* OLIVER J. LODGE, *D.Sc., Lecturer on Applied Mathematics and Mechanics at Uuiversity College, London*[*].

[Plate VII.]

1. THE approximate theory of the flow of heat down a uniform rod heated at one end and exposed to cooling influences everywhere else was, I believe, given by Biot before the time of Fourier, and was also verified experimentally by him. The "constants" occurring in Biot's equation to the

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[*] An abstract was read on the 8th of February, 1879.

curve of temperature have long been known to be variable; and numerous experiments have been made to determine empirically in what manner they depend upon the temperature. Nevertheless Biot's method in its original form has been frequently employed by subsequent observers in order to compare the conductivities of different metals—by Ingenhousz for instance, by Despretz, and even in the far more accurate experiments of Professors Wiedemann and Franz. Principal Forbes, on the other hand, devised and executed a method which was quite independent of any equations to curves except those deduced from experiment; and by graphical and other laborious methods he determined both the absolute conductivity of wrought iron and its variation with temperature. But, as far as I know, no recalculation of the curve of temperature with the improved data now accessible has been made. It therefore seemed worth while to obtain as close an approximation to the equation of the true curve of temperature as is practicable without cumbrous integration, and to see how far the improvement affects the results of those experimenters who, unlike Forbes, depended on the theoretical curve of temperature. Moreover it seemed probable that the more accurate equation to the curve would enable the variation in conductivity of the rod with temperature to be calculated in some moderately simple manner, and with far less labour than that gone through by Forbes. Forbes's methods are perfect; the only objection to them is the excessive tediousness of the process of discussing the experimental results. And as it is a most important research at the present time to compare exactly the law of variation of thermal and electrical conductivity in the same piece of material, it seemed desirable to have some means of calculating the law of thermal variation from some simple experimental data; and the long-thin-rod form of experiment is evidently suitable for observing the variation of *electric* conductivity with temperature.

The following paper is unfortunately rather long; but the length is due to the necessity of fully discussing experimental results, and I have skipped nearly all the mathematical steps, as they are elementary and of no interest.

2. Consider a thin uniform infinitely long rod, of perimeter p and cross section q, made of a material whose specific con-

ductivity is k, density ρ, and specific heat c. Let this rod be surrounded by an enclosure at the absolute temperature v_0, and let one point of the rod (which we will call the origin) be kept by some means at a constant temperature Θ above that of the enclosure; then heat will flow from this point along the rod and will be dissipated at its surface, and the temperature of every point of the rod will rise at a rate proportional to the excess of the quantity of heat which it gains per second by conduction, over that which is dissipated by radiation and convection. After a long time, however, this excess of heat vanishes, and the temperature of any point of the rod ceases to rise, having attained a constant temperature θ above that of the enclosure—its absolute temperature, t, being therefore $\theta + v_0$. (I will adhere to the letter v for temperatures reckoned in Centigrade degrees from absolute zero, and to θ for temperatures reckoned from the temperature of the enclosure as zero. We shall have to use occasionally the Centigrade zero—the temperature of melting ice; and temperatures reckoned from it may be denoted by t.)

The heat which flows in unit time past any cross section of the rod at a distance x from the origin will be

$$-kq\frac{d\theta}{dx};$$

and the gain of heat per second by an element of volume qdx in this position will be the differential of this quantity, or

$$kq\,d\frac{d\theta}{dx}.$$

If every unit area of the surface of the rod at this point is losing by radiation and convection the quantity H per second, the rate of loss of heat by the surface of the element is

$$\mathrm{H}pdx,$$

the product pdx being the area of its surface. As long as the temperature of the element is rising, the rate of rise of temperature will be the difference of the last two expressions divided by the thermal capacity of the element—that is, divided by $c\rho qdx$; but when the permanent state is reached, the heat gained and the heat lost become equal, and their equality is the fundamental differential equation for the permanent state of a rod, viz.

$$kqd\frac{d\theta}{dx}=\mathrm{H}pdx,$$

or

$$\frac{d^2\theta}{dx^2}=\frac{\mathrm{H}p}{kq}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The four quantities which enter into the right-hand side of this equation are all variables, and may be expressed as functions of θ. It has, however, been always assumed, in the approximate theory hitherto used, that H is the only variable, and that it is simply proportional to the excess of temperature, and can be written

$$\mathrm{H}=h\theta,$$

where h is a constant. (This is called Newton's law.) What we now want to do, however, is to take into account the variability of all these constants as far as present experimental results will enable us to do so, and then to integrate the above equation to as great a degree of accuracy as is easily possible.

3. Now, if an isolated body of volume V, surface S, density D, and specific heat C loses from each unit of surface a quantity of heat H per second, then its rate of fall of temperature is

$$-\frac{dv}{d\tau} \text{ or } \dot{v}=\frac{\mathrm{SH}}{\mathrm{VDC}};$$

writing $\dot{v}$ or $\dot{\theta}$ for the essentially positive quantity $-\frac{dv}{d\tau}$. Hence an element of the rod (§ 2), if isolated from its neighbours by two flat impervious films, will cool at the rate

$$\dot{\theta}=\frac{\mathrm{H}pdx}{c\rho\, dx},$$

whence its rate of loss of heat per unit of surface is

$$\mathrm{H}=\frac{qc\rho}{p}\dot{\theta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Substituting this value of H in equation (1), it becomes

$$\frac{d^2\theta}{dx^2}=\frac{c\rho}{k}\dot{\theta}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which is precisely the *form* of the equation to the variable flow of heat through a slab*, though $\dot{\theta}$ has there a very different meaning. The product $c\rho$ is the capacity for heat of unit volume of the rod (ρ being the mass of unit volume); hence $\frac{k}{c\rho}$ is the conductivity in terms of a unit of heat which can raise unit volume of the rod one degree. This Professor Maxwell calls the *thermometric* conductivity*, as distinguished from the calorimetric conductivity k.

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* See Everett, Trans. Roy. Soc. Edinb. vol. xxii.

4. In equation (3), $\dot{\theta}$ is a function of θ; and if the element were not supplied with heat, it would cool at the rate $\dot{\theta}$, and both θ and $\dot{\theta}$ would be functions of the time. But when heat is supplied to the element at a compensating rate by its neighbours, θ is constant, and therefore also $\dot{\theta}$ is constant as regards time; yet still the rod will emit heat at the same rate H as before, and $\dot{\theta}$ will be the same function of θ as if it were actually cooling: hence $\dot{\theta}$ was called by Forbes the *statical* rate of cooling.

The relation between $\dot{\theta}$ and θ for a cooling body, or the curve which expresses $\dot{\theta}$ as a function of θ, has been investigated experimentally by Dulong and Petit, and found to be of an exponential form. Newton's law made it a straight line. Forbes called it the secondary curve of cooling, and found a point of inflection on it for a long body cooling in air. For a rod in a permanent state, θ is a function of x; and the curve θ, x is the statical curve of temperature down the rod, and is the one we want to investigate. The curve $\dot{\theta}, x$ is what Forbes called the statical curve of cooling. Finally, the curve expressing θ as a function of time is the ordinary curve of cooling of a body. The general nature of these last three curves is the same, and depends on that of the first curve $\dot{\theta}, \theta$. The first rough approximation to them is that they are all logarithmic, this being a consequence of the hypothesis that the first curve is a straight line. I suppose that the fact that $\dot{\theta}$ is only *apparently* a function of the time renders abortive the analogy between equation (3) and the equation to the variable flow of heat in a slab.

On the Variation of $\frac{k}{c\rho}$ *with Temperature as at present known.*

5. Professor Tait has given theoretical reasons for assuming

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* See Maxwell's 'Theory of Heat,' p. 235.

the conductivity (*i. e.* the *thermometric* conductivity) of every substance to be inversely proportional to the absolute temperature*; but I do not know whether he lays much stress upon the correctness of the theory. At any rate it does not seem to agree very well with the results of experiment, except in the case of iron. The experiments of Principal Forbes† established the fact that the thermometric conductivity of wrought iron is nearly inversely proportional to the absolute temperature; but the agreement is not quite perfect, as the following Table shows.

Centigrade temperature, t.	Conductivity at the temp. t, as found by Forbes, reduced to C.G.S. units, $\frac{k}{cp}$.	Product of conductivity and absolute temperature, $(274+t)\frac{k}{cp}$.	Product which is more nearly constant, $(400+t)\frac{k}{cp}$.	Product, $(308+t)\frac{k}{cp}$.
0°	·2331	63·87	93·24	72·26
50	·1995	64·65	89·78	71·82
100	·1764	65·98	88·20	72·32
150	·1629	69·08	89·60	74·93
200	·1528	72·41	91·68	
250	·1440	75·43	93·60	

The third column contains the numbers which ought to be constant if the theory were accurate. The fourth column contains numbers calculated on the hypothesis that the conductivity varies inversely as the absolute temperature increased by some constant, say by 126. These numbers agree with one another rather better than those in the preceding column; but still there is a regular divergence perceptible between the hypothesis and the experimental results, especially at high temperatures.

The results at the higher temperatures, however, do not seem to have been regarded by Principal Forbes as equally dependable; for he gives an empirical formula for the conductivity at any Centigrade temperature t which does not agree very closely with the experimental results at high temperatures, saying, "I have assumed that the most trustworthy part of the observational curves are those between the actual temperatures of 40° and 160°, and that within moderate limits the conductivity may be represented in terms of the temperature by such a formula as"

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* See 'Recent Advances,' p. 271.

† Trans. Roy. Soc. Edinb. vols. xxiii. & xxiv.

$$\frac{k}{cp}=A+Bt+Ct^2,$$

where the constants for the best of his two bars, when reduced to the C.G.S. system, are

$$A=\cdot 2331,\quad B=-\cdot 00755,\quad C=\cdot 00000189.$$

6. Forbes's formula may therefore be written

$$\left(\frac{k}{cp}\right)_{Fe}=\cdot 2331(1-\cdot 00324t+\cdot 0000081t^2);$$

and this may be very accurately expressed by a form more suitable for our present purpose, $\frac{A}{b+t}$. For this last may be regarded as the sum of an infinite geometrical progression with ratio $-\frac{t}{b}$, and may be written, without approximation,

$$\frac{A}{b+t}=\frac{A}{b}\left(1-\frac{t}{b}+\frac{t^2}{b^2}-\ldots+\frac{t^n}{(b+t)b^{n-1}}\right),$$

stopping at any term one likes, and multiplying it by $\frac{b}{b+t}$ instead of writing the remaining terms.

Now if T be the highest temperature to which the formula is required to apply, the average temperature $\frac{1}{2}$T may be introduced into the denominator of the last term instead of the variable t, without making much difference; and the above may be written approximately, stopping at the third term,

$$\frac{A}{b+t}\triangleq\frac{A}{b}\left(1-\frac{t}{b}+\frac{t^2}{b(b+\frac{1}{2}T)}\right)^*.$$

This expression agrees very well with Forbes's formula for a range of 200°; for taking $b=308$, $\frac{1}{2}T=100$, and $\frac{A}{b}=\cdot 2331$, it becomes

$$\frac{A}{308+t}=\cdot 2331(1-\cdot 00325t+\cdot 000008t^2).$$

Hence I shall assume that the results of Forbes may be

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[*] The symbol $\triangleq$ is used merely to signify approximate equality.

summed up in the equation

$$\left(\frac{k}{c\rho}\right)_{\mathrm{Fe}}=\frac{\mathrm{A}}{308+t}=\frac{308}{308+t}\cdot\frac{k_0}{c_0\rho_0}. \quad . \quad . \quad . \quad (4')$$

The numbers in the last column of the preceding Table give the values of the "constant" A, for the temperatures considered most trustworthy by Forbes.

7. The variations of conductivity with temperature have also been investigated, by a method depending on fluctuating temperature, for copper and iron by Ångström*; and his result for iron, reduced to the C.G.S. scale, can be expressed in the following way,

$$\left(\frac{k}{c\rho}\right)_{\mathrm{Fe}}=\cdot2143(1-\cdot002874t),$$

which may be considered as equivalent to $\frac{\mathrm{A}'}{348+t}$ for a small range of temperature.

The results of Ångström for copper are summed up in the following formula,

$$\left(\frac{k}{c\rho}\right)_{\mathrm{Cu}}=1\cdot163(1-\cdot001519t),$$

which may be expressed as $\frac{\mathrm{B}}{658+t}$ when t is small.

8. The law of variation of the ordinary or calorimetric conductivity k can, of course, be deduced from the above by multiplying them by the value of the product $c\rho$, with each of its factors expressed as a function of the temperature—the one factor from the experiments of Bède on specific heat, the other from the expansion experiments of Fizeau. This Professor W. Dumas† has done. Using a mean coefficient of expansion between 0° and 100° C., he writes for the density of iron,

$$\rho=7\cdot7799(1-\cdot00003684t);$$

and for its specific heat,

$$c=\cdot1053(1+\cdot001348t).$$

Multiplying Ångström's value of $\frac{k}{c\rho}$ for iron by the product of these two quantities, we obtain

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* Phil. Mag. vol. xxv. 1863.

† Pogg. *Ann.* cxxix. See also Wüllner, *Exp. Physik*, vol. iii. pp. 286 & 287.

$$k=\cdot 1862(1-\cdot 00156\,t).$$

And multiplying Forbes's numbers for $\frac{k}{cp}$ (nominally for k), as reduced to the C.G.S. system by Dr. Everett, on page 44 of his book on Units (first edition), by the variation factor $(1+\cdot 001311\,t)$, we obtain the real values of k at the different Centigrade temperatures t according to Forbes's experiments,

t.	k.
0	·207
25	·1975
50	·189
75	·182
100	·177
200	·171

numbers which can be expressed with considerable accuracy by the formula

$$k=\cdot 207(1-\cdot 00144\,t).$$

9. The result of all this appears, then, to be that, as far as experiment has hitherto gone, the conductivity (both calorimetric and thermometric) of the metals copper and iron may be expressed with moderate correctness as a linear function of the Centigrade temperature, with a negative value for $\frac{dk}{dt}$, or

$$\frac{k}{cp}=\mathrm{A}-\mathrm{B}t,$$

and that it may be expressed with a trifle more accuracy by an inverse function of the temperature,

$$\frac{k}{cp}=\frac{\mathrm{A}}{b+t},$$

because this may be written very approximately, when b is much bigger than t,

$$\frac{\mathrm{A}}{b}\left(1-\frac{t}{b}+\frac{t^2}{b^2}\right).$$

I will therefore assume that the variation of conductivity in any metal for moderate ranges of temperature is expressed by the equation

$$k=\frac{\mathrm{A}cp}{b+t}, \quad \ldots\ldots\ldots \quad (4)$$

and that, since the variations of density and specific heat are known, the law of variation of conductivity in different metals is sufficiently discovered as soon as we have found the value of the constant b for each metal. Our object then is to find a mode of calculating b.

On the Variation of $\dot{\theta}$ with Temperature.

10. We have now to consider in what way the other factor of the right-hand side of equation (3), namely $\dot{\theta}$, may be expressed as a function of the temperature.

The magnificent researches of Dulong and Petit on this point have established the following expression for the velocity of cooling of a body whose absolute temperature is v, in an enclosure of absolute temperature v_0, containing gas at the pressure ϖ,

$$\dot{v} = \mathrm{P}(a^v - a^{v_0}) + \mathrm{Q}\varpi^g(v - v_0)^{1\cdot232},$$

which may also be written in terms of the excess of temperature $\theta = v - v_0$, thus,

$$\dot{\theta} = \mathrm{P}a^{v_0}(a^\theta - 1) + \mathrm{Q}\varpi^g\theta^{1\cdot232}. \quad . \quad . \quad . \quad . \quad (5)$$

The first term is the rate of cooling by *radiation*; the second term is the rate of cooling by *convection*. In other words, $\mathrm{Q}=0$ in a vacuum; and P is small if the surface of the body is silvered, but great if it be lampblacked. The constant g depends on the nature of the gas; for air it is ·45; but a is said to be a universal constant, and equal to 1·0077. Although this is an empirical formula, it is perhaps the most perfect example of such a formula that we have, and it expresses Dulong's results thoroughly well. The necessity of such an elaborate expression has been called in question by Narr; but his experiments, so far as they go, seem rather to confirm than to upset this expression; and it has been in the main verified by Provostaye and Desains.

11. Some caution, however, seems advisable with respect to the second term, which expresses the loss by convection as a constant power of the excess of temperature; because it was found by Principal Forbes that when the excess of temperature was very small, the loss by convection was almost inappreciable; and he suggested the viscosity of the air to account for this—some finite excess of temperature being required to

set convection-currents going. The point of inflection, moreover, which Forbes found on the curve $\dot{\theta}$, θ at a high temperature (see § 4) is wholly unaccounted for by Dulong's expression; but it is probable that here the experiments of Dulong are the most accurate, and that the contrary flexure of Forbes's curve was due to waves of heat in the elongated mass whose cooling he investigated*. Experiments on the rate of cooling of bodies at the ordinary pressure of the atmosphere have been made by Mr. Macfarlane and by Mr. Nichol; but their excesses of temperature only went as high as 60° (see Everett, 'On C.G.S. Units,' p. 50). I believe Professors Ayrton and Perry have some results not yet published.

On another ground also caution seems to be rendered necessary by the kinetic theory of gases, as illustrated in the experimental investigations of Mr. Crookes; for if the enclosure containing the cooling body be gradually exhausted of air, so that ϖ progressively diminishes, a discontinuity, in the direction of a sudden increase in the rate of cooling, would probably arise at the instant when the average free path of the molecules was long enough to reach from the surface of the cooling body to that of the enclosure. And it is probable that for exhaustions higher than this the law of cooling is different, and in all probability simpler than it was when the heat had to be conveyed between the surfaces by the unsystematic and irregular agency of convection-currents, a process of true gaseous conduction then setting in. This is a point which should be attended to in subsequent investigations; and it would be an important though somewhat difficult research to discover experimentally the law of cooling and its alteration with pressure when the distance between the cooling body and enclosure is less than the free path of the molecules: probably it could be more readily deduced from theory. It is not likely, however, that any of the investigators on the law of cooling hitherto have attained an exhaustion any thing like so perfect as this.

12. These objections, however, only apply to the *convection* part of the formula (5); and I will assume that the radiation part

$$\dot{\theta} = Pa^{v_0}(a^{\theta} - 1) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5')$$

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* Or see Professor Tait's explanation given to the Royal Society of Edinburgh on the 20th of last January ('Nature,' No. 486, p. 379).

is practically true as it stands. Since this, however, is not a very simple function for a second differential coefficient like (3), it will be well to see with what amount of accuracy we may expand it into a series and neglect higher terms. The expansion is

$$\dot{\theta}=\mathrm{P}a^{v_0}\left(\theta\log a+\frac{1}{2}(\theta\log a)^2+\frac{1}{6}(\theta\log a)^3+\frac{1}{24}(\theta\log a)^4+\ldots\right) \quad (6)$$

which may be conveniently written

$$\dot{\theta}=\tfrac{1}{2}\mathrm{P}a^{v_0}(\log a)^2\cdot\theta\left(\frac{2}{\log a}+\theta+\frac{1}{3}\theta^2\log a+\frac{1}{12}\theta^3(\log a)^2+\ldots\right),$$

or, putting in the numerical value of a, viz. 1·0077 (that is, putting $\log_e a=\cdot0076$),

$$\dot{\theta}=\mathrm{C}\theta(266\cdot6+\theta+\cdot0025\,\theta^2+\cdot000005\,\theta^3+\ldots),$$

or

$$\dot{\theta}=\mathrm{C}\theta\left(267+\theta+\frac{\theta^2}{400}+\frac{\theta^3}{200{,}000}+\frac{\theta^4}{18\times10^8}+\ldots\right).$$

Remember that θ is to be ultimately the excess of the temperature of any point of the rod over that of the enclosure. It may be any thing between 0° and 150°; but it is not likely in ordinary experiments to go above 200°. The terms of the above series for the extreme case $\theta=200$ are

$$267+200+100+40+1,$$

where only the last term, containing the fourth power of the temperature, can be regarded as quite negligible. But for the more likely case of $\theta=100$, the terms of the series are

$$267+100+25+5+\frac{1}{18},$$

where the term containing the cube of θ is not of much consequence. If, however, it were wished not to go higher than the second power, the term containing the cube need not be neglected, but a mean value of it may be added to the coefficient of the second power of θ. Thus if Θ be the highest temperature taken notice of (*i. e.* the temperature of the origin in the case of the rod, the initial temperature in the case of a cooling body),

$$\dot{\theta}=\mathrm{C}\theta\left\{267+\theta+\theta^2\left(\frac{1}{400}+\frac{\frac{1}{2}\Theta}{200{,}000}\right)\right\};$$

and this is the expression we shall use, writing it first in the simpler form,

$$\dot{\theta} = C\theta\left\{267+\theta+\frac{\theta^2}{400}\left(1+\frac{\Theta}{1000}\right)\right\}. \quad . \quad . \quad . \quad (7)$$

Notice that θ occurs in this expression as a factor, so that it is really a cubic function of θ.

It is singular how near the constant term in these brackets is to the number 274. I suppose this is accidental; but at first sight it looked as if the rate of cooling for small excesses of temperature were proportional to the product of absolute temperature and excess, or as if the quotient $\frac{\dot{\theta}}{v\theta}$ would be constant. On this hypothesis, however, the constant a, twice the reciprocal of whose logarithm is the number which happens to be nearly 274, would vary with v_0 the temperature of the enclosure, which is contrary to Dulong's results. Indeed there seems no ground for the conjecture.

Applying the correction for the neglected terms of the series, as is done in (7), we may write the expansion (6) thus, writing α instead of $\log_e a$ for shortness ($\alpha = {\cdot}0076$),

$$a^\theta - 1 \doteqdot \alpha\theta\left\{1+\frac{1}{2}\alpha\theta+\frac{1}{6}\alpha^2\theta^2\left(1+\frac{1}{8}\alpha\Theta\right)\right\}. \quad . \quad . \quad (7')$$

13. It remains now to show, from Tables of experimental results, to what amount of accuracy θ, multiplied by a quadratic function of θ, will represent the observed rate of cooling of a body in a vacuum.

And first I will take the experiments of Narr* (see Wüllner, vol. iii. p. 254). The following Table contains the result of his experiments in a vacuum. The first column is the observed rate of cooling at the Centigrade temperature shown in the second column, the enclosure being at zero Centigrade. The third column contains the product of excess of temperature t and absolute temperature v, divided by the rate of cooling, to show how far this ratio is constant. These numbers are observed to decrease regularly, though slowly, and in a manner which has an obvious relation to the corresponding number in the preceding column; so that if twenty times that number be subtracted from each, the result will be very constant, as is shown in the last column.

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* Pogg. *Ann.* vol. cxlii.

$\dot t$.	t.	$(274+t)\frac{t}{\dot t}$.	$\frac{vt}{\dot t}-20t$.
3·26	115°	13720	11420
3·11	110	13580	11380
2·80	100	13360	11360
2·49	90	13160	11360
2·18	80	12990	11390
1·88	70	12810	11410
1·73	65	12740	11440

Hence

$$\frac{rt}{\dot t}-20t=\text{const}=11400.$$

We may write this,

$$20\dot t=\frac{274+t}{570+t}\cdot t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8')$$

or, approximately,

$$\dot t \bumpeq \frac{t}{11400}(274+t)\left(1-\frac{t}{570}\right),$$

which is of the form of equation (7), namely the excess of temperature t multiplied by a quadratic factor. The numerical value of the constants do not, indeed, agree well with those of Dulong, especially in the fact of the sign of the coefficient of t^2 being negative; but this is hardly to be expected, as Narr seems to have undertaken his experiments with the object of upsetting Dulong's results. Narr's experiments, moreover, do not extend over any thing like the range of temperature that Dulong and Petit's did.

14. If we apply the same process to the Table expressing the results of the latter experimenters in a vacuous enclosure at Centigrade zero, we shall find that the number $+30$ has to be used instead of -20; so that $\frac{vt}{\dot t}+30t$ is very tolerably constant, and equal to 18650 on the average, as is shown in the following abridged Table of Dulong's results. Considering that the range of temperature extends as high as 240°, the agreement is pretty good.

$\dot{\theta}$.	θ.	$\frac{(274+\theta)\theta}{\dot{\theta}}$.	$\frac{v\theta}{\dot{\theta}}+30\theta$.
10·69	240°	11530	18730
7·40	200	12810	18800
4·89	160	14200	19000
3·02	120	15650	18050
1·74	80	16270	18670

Hence we may write

$$30\dot{\theta}=\frac{274+\theta}{622-\theta}\cdot\theta, \quad . \; . \; . \; . \; . \; . \; . \quad (8'')$$

or, approximately,

$$\dot{\theta}=\frac{\theta}{18650}(274+\theta)\left(1+\frac{\theta}{622}\right),$$

which is the form of equation (7).

It may be hereafter convenient to know that an expression like (8′) and (8″) is capable of representing the law of cooling in a vacuum with great accuracy, viz.

$$\dot{\theta}=C\theta\,.\frac{A+\theta}{B-\theta}; \quad . \; . \; . \; . \; . \; . \; . \quad (8)$$

but for our present purpose I think the equation (7) will be the most convenient.

15. The agreement of equation (7), as it stands, with Dulong and Petit's results it is scarcely necessary to show by a Table, since the equation has been deduced by known approximation from their own statement which completely expressed them, and the value of the terms neglected for an excess of temperature so high as 240° is perfectly evident. Nevertheless I have made the calculation, and the values of the "constant" $\left(267+\theta+\frac{6}{5}\cdot\frac{\theta^2}{400}\right)\frac{\theta}{\dot{\theta}}$, or $\frac{1}{C}$, corresponding to the successive excesses of temperature 240°, 200°, 160°, 120°, and 80°, are 15265, 15865, 16489, 17086, and 16827. Hence the discrepancy between equation (7) and experiment is not *great* even for temperatures so high as 240°; while for a maximum temperature under 150° or so the discrepancy is practically *nil*.

[To be continued.]

VI. *On the Photographic Method of Registering Absorption-Spectra, and its Application to Solar Physics.* *By* Capt. W. DE W. ABNEY, *R.E., F.R.S.*

THERE are certain difficulties in registering the visible absorption-spectra as observed, dependent on the eye of the observer, and on his power of representing correctly what he sees; and it is owing to these deficiencies that curious mistakes have been made in endeavouring to draw absorption-phenomena. Up to the present time it has been, comparatively speaking, useless to attempt such registration by means of photography, owing to the fact that merely one part of the spectrum was impressionable by the silver salts employed as a sensitive medium. Since my discovery that silver bromide could be prepared in such a molecular state as to be sensitive to the whole spectrum (visible, ultra violet, and ultra red*), the difficulty in the employment of photography is done away with; and it should be taken into use as much as possible, so as to eliminate the errors of eye-observations. A natural objection would arise at first sight, viz. that for the different parts of the spectrum the sensitiveness of the silver compound is materially different, and that consequently the absorption at different parts cannot be well compared. The objection vanishes, however, at once, if ordinary precautions are taken; and as an illustration I will take a case.

The absorption of a violet (cobalt) glass was required to be registered photographically. A spectroscope having two prisms of 62° was judged to give sufficient dispersion; and a lens was used in the camera of a focal length of about 2 feet. This gave a spectrum about 4 inches long, including the visible and invisible radiations. The plate having been placed in the camera, the top half of the slit was shielded, and sunlight was reflected onto the bottom half for two minutes; the sunlight was diverted, and the absorbing medium (in this case violet glass) was placed in front of the slit†, the lower half covered up, and sunlight again reflected onto the top half of the slit for two minutes more. The plate was then developed, and a print taken from the negative. A scale of shade having been prepared, the following diagram was drawn from the measurements made with it.

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* Except those radiations of low amplitude and large wave-length which are emitted by bodies at ordinary temperatures.

† When this paper was communicated to the Physical Society, Prof. Macleod suggested that the absorption of a liquid might be better demonstrated if a wedge-shaped vessel containing it were placed in front of a

The top continuous curve of fig. I. shows the intensity produced on the plate by unscreened sunlight. The dotted line in the same figure shows the curve obtained when the cobalt glass is interposed.

FIG. I

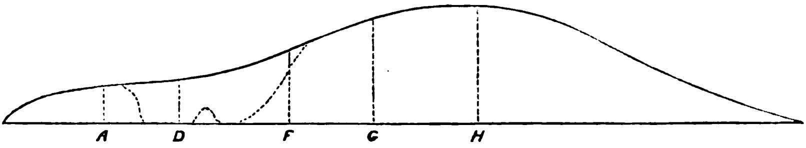
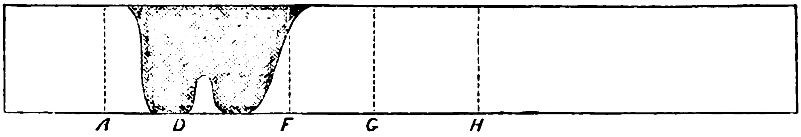


FIG. II



I would here remark that care is necessary not to introduce an error, as it must be remembered that the shades produced photographically have not the same gradations as the intensity of light, as Bunsen and Roscoe first showed.

Fig. II. shows the absorption of the violet glass, on the presumption that the intensity of the radiations is equal throughout the spectrum, an assumption which is very generally made.

I have found that it is convenient in taking these spectra to modify this method. The absorption produced by potassium chromate takes somewhat of a wedge-form, shading off from darkness in the violet to total transmission at the least-refrangible end of the spectrum. If a dilute solution of this

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longer slit, of which a small image might be produced at the focus of the collimating lens. This is quite practicable, as Professor Macleod and myself have found by actual experiment; and if the image of coloured liquid be corrected by a similar wedge of colourless liquid of nearly the same specific gravity, there is no inconvenience attaching to it.

substance be interposed in each case between the source of light and the slit for half the time of exposure, we have an impression of the spectrum the varying intensity of which is less marked than if such an artifice be not employed.

I may here remark incidentally that the passage of light through an aqueous solution seems to interfere very little with the intensity of the photograph at the least-refrangible end. I had looked for a marked diminution, but have scarcely noticed it.

In photographing these absorption-spectra the source of light should be brilliant: sunlight, the image of the incandescent points of the electric light, or the oxyhydrogen light, may all be used; but I prefer sunlight, as we are enabled by the Fraunhofer lines to fix the locale of the absorption-bands more readily than with the other two.

Another application of this method is to the solar spectrum itself. Researches have shown that the bright-line spectra of incandescent compound bodies should lie in the least-refrangible end of the spectrum, and that to discover these a search must be made in these regions. As far as the visible spectrum is concerned such a search has been made; but we have yet to examine those regions which are invisible. At a low temperature it is quite possible that the compound bodies should give off vapours of the compound, whilst at high temperatures, such as that of the electric arc, they are probably dissociated. If, then, we wish to ascertain the existence of such compounds in the photosphere, we are driven to compare the solar spectrum with the bright-line spectra of the various compounds when heated at such low temperatures as those of the ordinary colourless gas- or spirit-flame. To photograph portions of such spectra (even the most "actinic" region of the spectrum) is a feat of uncommon difficulty; and it would require hours, I might say days, of exposure to impress lines in the red-region. Such an attempt would be practically useless, as we can accomplish the same end in as many minutes by an indirect method as it would require hours by the direct method.

The following illustration will show how it can be accomplished. The top half of the slit is covered as before, and sunlight reflected onto it, and the spectrum is impressed on the photographic plate. The bottom half is next covered up, and a flame, in which the compound to be examined, is placed in front of the slit; the sunlight is then caused to traverse the flame, and a second spectrum is impressed on the plate through the top half of the slit.

New absorption-lines are thus formed in the solar spectrum, or *those already existent are intensified*, as is already well known. As an example, lithium chloride was heated in the flame, and the known line of lithium was found reversed between B and C, though absent in the spectrum of sunlight, and a faint line lying in the spectrum below the red was found intensified. By following out this plan we perhaps may eventually establish the existence of compounds in the solar photosphere. By using the light emanating from the white-hot carbon points of the magnetoelectric light to produce a continuous spectrum, and by burning the metallic compounds as before for one spectrum, and then by using sunlight to give the other spectrum, confirmatory evidence would be obtained. I may remark that I have photographed bright-line spectra of lithium, and got the same line in the ultra red as that obtained reversed. This method seems to promise to be a new weapon of attack in solar physics, more especially in this ultra-red portion.

VII. *On Spectra of Lightning.*

By ARTHUR SCHUSTER, *Ph.D., F.R.A.S.*[*]

ALL observers of lightning-spectra agree in having seen the line-spectrum of nitrogen; but most of them have seen, in addition to this, sometimes a continuous spectrum, sometimes a band spectrum, the chemical origin of which is unknown.

The following historical summary may give an idea of our knowledge on that point.

Prof. Kundt (Pogg. *Ann.* cxxxv. p. 315) observed a line spectrum consisting of one or two lines in the red, some very bright ones in the green, and some less bright ones in the blue. He mentions that the lines are not always seen together.

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[*] Read February 22nd.

Lines which in one flash appeared especially bright were not seen in another flash. The greater number of flashes, however, gave a different spectrum altogether. In the place of bright lines a great number of bands were seen; and Prof. Kundt even distinguishes two different band spectra.

Mr. John Herschel (Proc. R. S. xvii. p. 61) observed a variable continuous spectrum crossed by bright lines, which also had a variable intensity. He gives the measurements of two lines, which agree very well with nitrogen-lines.

M. Laborde (*Les Mondes*, viii. p. 219) observed some lines, especially one near E, which sometimes appeared alone. He also saw a continuous spectrum.

Dr. H. Vogel (Pogg. *Ann.* cxliii. p. 653) describes lines only; but in his list I find two which do not coincide with any bright lines in the spectrum of the electric spark taken in atmospheric air; they do, however, coincide with two bands which I have observed in some flashes of lightning, as I shall show.

Mr. J. P. Joule ('Nature,' vol. xvi. p. 161) also observed some spectra of lightning. Frequently there was only one bright line visible, this being coincident with the brightest nitrogen-line. At other times there were several bright lines visible, sometimes with and sometimes without the green nitrogen-line. A continuous spectrum was also observed.

Mr. H. R. Procter ('Nature,' vol. xvi. pp. 161 & 220) gives some measurements of lines which do not lay claim to any accuracy. He observed also a band spectrum, which he finds not to be the band spectrum of nitrogen.

From conversation with Prof. A. Young, I learned that he also had seen a line spectrum, a band spectrum, and a continuous spectrum.

During my stay in Colorado last summer, I had some good opportunities of studying the spectra of lightning. It was my intention to get some reliable measurements of the band spectrum which I, in common with most observers, have seen; and in order to have greater chance of succeeding, I confined myself to one part of the spectrum only. The part I chose extended from $\lambda = 5000$ to $\lambda = 5800$, and covered, therefore, most of the yellow and green. I used a direct-vision spectroscope, with a slit movable by means of a micrometer-screw. A bright line in the principal focus of the telescope formed a fiducial mark. Under ordinary circumstances, the slit is moved until the line to be measured forms a continuation of the bright line which reaches down into the centre of the field. I found, however, that the bands I wanted to measure were nearly as broad as the thin glass bar which carries the bright line; and I used the bar therefore simply as pointer. The measurements were always made at night; and the spectroscope was left undisturbed until the following morning, when the Fraunhofer lines in the neighbourhood were measured, so that the wave-lengths of the measurements could be interpolated.

It is of course impossible to put a pointer on a band during the instantaneous flash; but a succession of flashes allows us to put the pointer successively nearer and nearer until we see it in coincidence with the band. In this way several readings of each band were obtained. The dispersive power of the spectroscopes was such that, with a higher-power eyepiece than the one used in this investigation, the nickel-line could be seen between the two sodium-lines. The distance between the two sodium-lines was such that the two readings of the slit differed by ten divisions of the micrometer, or one tenth of a whole revolution. With sunlight I can measure easily to the tenth part of the distance between the sodium-lines. I obtained measurements on three different nights. Unfortunately, the best nights for the work occurred before the Total Solar Eclipse, which had taken me out to Colorado. A desire to save my eyes prevented me from making as good use of these nights as I otherwise should have done.

July 25th, West Las Animas.—The whole horizon seemed to be almost constantly illuminated with lightning, generally sheet-lightning. I observed about thirty or forty different flashes. I often saw the bright nitrogen-lines 5002 and 5681. I did not take any measurements of these lines; but there can hardly be a doubt as to their position. I saw in the part of the spectrum which I was observing three bands, which, however, did not always appear together. The measurements reduced to wave-lengths will be given further on. Two measurements of the bands β and γ were obtained, but one only of the band α. The greatest difference between the two measurements amounts to three times the distance between the sodium-lines. This difference must be partly accounted for by the difficulty of the observation, partly by the fact that the spectroscope had only just been unpacked after the journey; and it was found next day that it was considerably out of adjustment. The micrometer-screw, also, owing to the heat and dust, had a considerable backlash; it was taken to pieces next day and cleaned, which greatly improved it.

August 3rd, Manitou.—Clouds were coming from the west over Pike's Peak; and strong flashes of lightning, partly sheet lightning, partly forked lightning, were observed. Only two measurements were secured. One of the bands measured was β. Prof. Arthur Wright, who was present, observed that one spot of the sky was illuminated during some flashes with a strong blue light, looking like a fluorescent light. I pointed the spectroscope to that spot, and observed a single broad band in the green. I moved the pointer on it as well as I could; but not being able to get another flash to verify the measurement, I had to take the reading. The position of this band, which I call δ, is very doubtful.

August 18th, Salt-Lake City.—I only obtained one measurement of the band γ. The kind of lightning observed differed considerably from that of the preceding nights. The lightning was nearly all forked lightning; and the bright nitrogen-line came out very strongly. The bands were but seldom seen. In one flash I saw a series of lines in the green which I had never seen before. My impression is that they were at about equal distances from each other, decreasing in strength towards the red; so that the whole made an impression similar to that of a fluted band, such as those seen in the spectrum of aluminium oxide, but shading off towards the red.

In addition to the line and band spectra, I have on many occasions seen a continuous spectrum only.

The following Table contains all the measurements I have taken. I have added in the last column numbers contained in Dr. Vogel's list of lines. It will be seen that these coincide with two of the bands I have seen.

Band.	Date.	λ	Mean.	Vogel.
a.	July 25	5592	5592	
β.	July 25 July 25 Aug. 3	5348 5329 5325	5334	5341
γ.	July 25 July 25 Aug. 18	5175 5193 5177	5182	5184
δ.	Aug. 3	5260	5260	

In trying to identify these bands with known spectra we meet with an unexpected difficulty. Two of them unfortunately admit of two different interpretations. At first sight I was struck by the close agreement of *α* and *γ* with two bands of carbonic oxide. These bands fade away towards the blue; and their sharp edges have a wave-length of 5607 and 5197. Observing with the same spectroscope, and widening the slit as I did in observing the lightning, I can produce the same impression of an unshaded band; and taking a measurement of the centres of the bands under these circumstances, I obtain $\lambda = 5579$ and $\lambda = 5180$, which agree within the limits of possible errors with the above values.

The ordinary spectrum of air, however, contains a band at 5178; so that, as far as mere position is concerned, one might well be taken for the other. I was, however, under the impression that I had sometimes seen this band without the chief nitrogen double line 5002–5; and as the yellow band of carbonic oxide was also apparently present, I stated with considerable confidence when I first wrote out this paper that I had observed the spectrum of carbonic oxide. It was only when I came to work out the position of the band *δ* that I began to have serious doubts as to the accuracy of this conclusion. The position of the band *δ*, as I have said, is very doubtful; I even thought it was possible that I had taken a very bad measurement of either *β* or *γ*, and felt at first inclined to reject it altogether. On working out its wave-length, however, I found that it was coincident with one of two strong bands, which are found at the negative pole of vacuum-tubes filled with oxygen. Now the second of the two bands is nearly coincident with the yellow band of carbonic oxide; so that, of the two bands which I at first thought belonged to that gas, one might be due to nitrogen, the other to oxygen, as seen at the negative pole.

The explanation of the band β is obvious. It is the brightest of the two green lines in the low-temperature spectrum of oxygen. Its wave-length, when seen under a pressure of about a millimetre, is 5329; but under higher pressures it widens more on the less-refrangible side than towards the blue, and may well appear as a band with its centre at 5334 or even 5341, as given by Vogel.

I have not been able to obtain this band from atmospheric air in vacuum-tubes, although I have tried the experiment under various pressures. If the so-called continuous discharge is allowed to pass, the band spectrum of nitrogen alone appears; if the disruptive discharge passes, the high-temperature spectrum of oxygen is superadded to the line spectrum of nitrogen. As regards the two bands α and γ, it does not seem to me to be possible at present to decide between the two interpretations which I have given. On the one hand, it seems improbable that the slight traces of carbonic acid known to exist in the atmosphere should reveal their presence in the spectrum; but, on the other hand, it is to be remarked that oxygen vacuum-tubes, which show the band β, always reveal the slightest trace of carbonic oxide. It is exceedingly difficult, though quite possible, to obtain the band β without the bands α and γ. The measurement of δ, however uncertain, renders it probable that the spectrum of the negative pole in oxygen forms part of the spectrum of lightning; and on the whole I should feel inclined to attribute the band α to oxygen. I have shown in my paper on the spectrum of oxygen that this spectrum of the negative pole is due to an allotropic modification of oxygen (possibly ozone), and I have been able to obtain it, though only temporarily, in the positive part of the discharge. As regards the band γ, I have some difficulty in attributing it to nitrogen, and still think it probably due to carbonic oxide. During the observations I certainly felt convinced that it did not belong to the same spectrum as the chief lines of nitrogen, and I made a note that, on the contrary, it generally appeared together with β. It seemed sometimes to be present alone, and often to form the most prominent part of the whole spectrum. As the lines of the capillary part of an oxygen-tube are also present at the negative pole, together with the bands distinctive of that pole, I can best express my observations on the band spectrum of lightning by saying that it resembles in a remarkable way the spectrum which is found at the negative pole of a vacuum-tube filled with oxygen which is slightly contaminated with carbonic oxide.

VIII. *On the Transmission and Distribution of Energy by the Electric Current.* *By* C. William Siemens, *D.C.L., F.R.S.*[*]

[Plate VIII.]

In the autumn of 1876, when standing below the Falls of Niagara, the first impression of wonderment at the imposing spectacle before my eyes was followed by a desire to appreciate the amount of force thus eternally spent without producing any other result than to raise the temperature of the St. Lawrence a fraction of a degree[†], by the concussion of the water against the rocks upon which it falls.

The rapids below the fall present a favourable opportunity of gauging the sectional area and the velocity of the river; and from these data I calculated that the fall represents energy equivalent to nearly 17 million horse-power, to produce which by steam would require about 260 million tons of coal a year, or just about the entire amount of coal raised throughout the world.

If one fall represents such a loss of power, what must be the aggregate loss throughout the world from similar causes? and is it consistent with utilitarian principles that such stores of energy should go almost entirely to waste? But the difficulty arises, how such energy (occurring as it does for the most part in mountainous countries) is to be conducted to centres of industry and population.

Transmission by hydraulic arrangements or by compressed air would be very costly and wasteful for great distances; but it occurred to me that large amounts of energy, produced by means of the dynamo-electric current-generator, might be conveyed through a metallic conductor, such as a rod of copper fixed upon insulating supports. Such a conductor would no doubt be expensive; but, if once established, the cost of maintenance would be very small, and its power of transmitting electric energy would be limited only by the heat generated in it through electric resistance.

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[*] Read February 22nd.

[†] The vertical fall being 150 feet, the increase of temperature would be $\frac{150}{772} = \frac{1}{5}^\circ$ Fahrenheit nearly.

In venturing to give expression to my thoughts upon this subject, in my address to the Iron and Steel Institute in March 1877, I stated that a copper rod 3 inches in diameter would be capable of transmitting energy to the extent of a thousand horse-power an hour a distance of 30 miles, there to give motion to electrodynamic engines, or to produce illumination sufficient to light up a town with 250,000 candle-power.

Although this statement was considered by many a bold one at the time it was made, I now find that a conductor such as I then described might be able to transmit three or four times the amount of power then named, and that the light producible per horse-power was also, according to our present more advanced state of knowledge, very much understated.

No serious difficulty need be apprehended as to the production of a current sufficient in amount to fill a conductor of such large proportions as here indicated. Although it would perhaps be impossible to construct a single dynamo-electric machine of sufficient power for that purpose, any number of smaller machines could be easily coupled up both for intensity and quantity to produce the desired aggregate amount.

A difficulty would, however, arise at the other end, where the electric energy was to be applied, and where it would therefore be requisite to have an arrangement for its distribution over a number of branch circuits, so that each might receive such a proportion of the total current in the main conductor as to produce the number of lights, or the amount of power intended to be supplied. An accidental increase of resistance in one or other of the branch circuits would produce the double inconvenience of starving the circuits in which such increased resistance had occurred and of supplying an excess of current to the other circuits.

In order to carry out such a system of supply, it would be necessary to have the means of so regulating the current in each branch circuit, that only a predetermined amount should be allowed to flow through the same; it would be desirable also to furnish each circuit with the means of measuring and recording the amount of electric current passed through the same in any period of time.

It is my special purpose to bring before you an instrument by which these two purposes can be accomplished. The current-regulator (as represented in Plate XII.) consists principally of a strip of metal (of mild steel or fused iron by preference), which by its expansion and contraction regulates the current passing through it. This strip is rolled down to a thickness not exceeding 0·05 millim., and is of such a breadth that the current intended to be passed through the regulated branch circuit would raise the temperature of the strip to say 50° C.

This strip of metal (A) is stretched horizontally between a fixed support and a regulating-screw (B), at which latter the current enters, passing through the strip, and thence through a coil of German-silver wire (C) laid in the form of a collar round the centre, and connected at its other extremity with a binding-screw (D), whence the current flows on towards the lights or other apparatus to be worked by electricity. Upon its middle the strip carries a saddle of insulating material, such as ebonite, upon which rests a vertical spindle, supporting a circular metallic disk (E), with platinum contacts arranged on its upper surface. Ten or any other number of short stout wires connect the helical rheostat at equidistant points with adjustable contact-screws (F), standing above the platinum contacts on the surface of the metallic disk. These wires are supported upon the circular frame (G) of wood or other insulating material, but are free to be lifted off their support if the metallic disk should rise sufficiently to be brought into contact with the screws. These latter are so adjusted that none of them touches the metallic disk when it is in its lowest position, but that they are brought one after another into contact with the same as the disk rises; and it will be easily seen that for every additional contact-screw that is raised *seriatim* by the disk, a section of the helical rheostat between attachment and attachment is short-circuited by the metallic disk, and thus excluded from the circuit. When the disk is in its uppermost position the whole of the rheostat is short-circuited, and the regulator offers no other resistance to the current than that of the horizontal strip itself. In setting the regulator to work the regulating-screw (B) is drawn on sufficiently to bring the whole of the contact-screws into contact with the disk. The passage of the current through the strip will have the effect of raising its temperature to an extent commensurate with the electrical resistance; and in the same measure the strip itself will be elongated, and cause the spindle with the contact-disk to descend.

Another form of this instrument depends for its action upon the circumstance discovered by the Count du Moncel in 1856, and more recently taken advantage of by Mr. Edison, that the electrical resistance of carbon varies inversely with the pressure to which it is subjected. A steel wire of 0·3 millim. diameter is attached at one end to an adjusting-screw, B, and at the other to one end of a bell-crank lever, L, by means of which the pressure is brought to bear upon a pile of carbon disks, C, placed in a vertical glass tube. The current enters the instrument at the adjusting-screw B, and, passing through the wire and bell-crank lever, leaves below the pile of carbon disks. Its effect is to cause a rise of temperature in the steel wire, which, through its expansion, diminishes the pressure upon the carbon disks, and thus produces an increase in their electrical resistance. This simple apparatus thus supplies a means of regulating the strength of small currents, so as to vary only within certain narrow limits.

According to Joule's law the heat generated in the strip per unit of time depends upon its resistance, and upon the square of the current; or

$$H=C^2R, \therefore C=\sqrt{\frac{H}{R}}.$$

On the other hand, the dissipation of heat by radiation depends upon the surface of the strip, and upon the difference between its temperature and that of the air. Therefore, in order that the current C may remain constant, it must, at every moment, be equal to the square root of the temperature divided by the resistance; and this function is performed automatically by the regulator, which throws in or takes out resistance in the manner described, according as the temperature increases or diminishes.

The regulating instrument may also be adapted to the measurement of powerful electric currents, by attaching to the end of the sensitive strip a lever, with a pencil pressing with its point upon a strip of paper drawn under it in a parallel direction with the lever by means of clockwork, a datum line being drawn on the strip by another pencil. The length of the ordinate between the two lines depends, in the first place, upon the current which passes at each moment, and, in the second place, upon the loss of heat by radiation from the strip.

If R′ is the resistance and H′ the heat with a current C′ and temperature T′, then, by the law of Joule,

$$H' = R'C'^2,$$

and the loss by radiation is equal to

$$H' = (T' - T)S,$$

in which T′ is the temperature of the strip, T that of the atmosphere, and S the surface of the strip.

Considering that the resistance varies as the absolute temperature of the conductor, according to a law first expressed by Helmholtz, the value of R may be put for R′ for small variations of temperature; and as during an interval of constant current the heat generated and that radiated off will be equal, we obtain

$$C'^2 = (T' - T)\frac{S}{R'}, \therefore C' = \sqrt{\frac{(T' - T)S}{R}} \quad . \quad . \quad . \quad (1)$$

in which T′ − T represents the movement of the pencil, and S is constant.

For any other temperature T″,

$$C'' = \sqrt{\frac{(T'' - T)S}{R}}.$$

For small differences of C″ and C′,

$$(C'' - C')^2 = 2C''(C'' - C');$$

that is to say, small variations of current will be proportional to the variations in the temperature of the strip.

To determine the value of a diagram in Weber's or other units of current, it is only necessary, if the variations are not excessive, to average the ordinates, and to determine their value by equation (1), or from a Table.

These observations may suffice to show the possibility of regulating and measuring electric currents with an ease and certainty quite equal to that obtained in dealing with currents of liquids such as gas or water; and the time may not be far distant when the use of such an instrument will also become a public necessity.

Other forms of the instrument will readily suggest themselves to the mind of the constructive engineer; but the two typical forms I have described on this occasion will suffice, I think, to show its general character.

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OF

THE PHYSICAL SOCIETY

OF LONDON.

JULY 1879.

IX. *A new Theory of Terrestrial Magnetism.*

By Professors JOHN PERRY *and* W. E. AYRTON*.

IN the autumn of 1876, while experimenting on magnetic transparency, we designed an apparatus for testing whether a moving body having a definite electric charge would, like a current, deflect a magnet. While waiting for the conclusion of the rains, and the advent of the very dry season which accompanies a Japanese winter, in order to try our instrument in conjunction with an ordinary plate-glass electrical machine, we received the account, published in the Philosophical Magazine for September 1876, of the experiments just performed by Mr. Rowland in the laboratory of Professor Helmholtz, by which it had been conclusively shown that a charge of electricity mechanically moved had the properties of an ordinary electric current as far as the deflection of a magnet was concerned†.

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* Read March 8.

† Additional confirmation has recently been given on this subject by the experiments described by Mr. Crookes in his paper on the "Illumination of the Lines of Molecular Pressure &c.," since he has shown that the stream of particles which is shot off from the negative terminal in a very perfect vacuum, and which produces the green phosphorescence, carries electricity with it and is deflected by a magnet. This may be regarded as a sort of converse experiment, since it proves that a magnet deflects a moving charge of electricity.

Until this point was settled, it was unlikely that attention would be directed to the electromagnetic effects that might arise from the rotation of a charged body like the earth. Shortly, however, after the execution of the experiments referred to we attempted (as described in our paper "On Rain Clouds and Atmospheric Electricity," which appeared in the Philosophical Magazine for March 1878) the solution of a new theory of terrestrial magnetism. This problem we have attacked in a variety of ways; and the following solution, to which we have at length been led, we beg to offer for the acceptance of the Physical Society.

The points near the surface of the earth have different linear velocities from those in the interior (although all the points have the same angular velocity of rotation round the earth's axis); therefore, if the earth had an initial electrical charge, residing of course, in accordance with the well-known electrical law, on its surface, the electrified particles would have velocities relative to the remainder; hence, as a direct consequence of the results of the experiments published by Professor Helmholtz, the interior of the earth would be a magnetic field, quite independently of its interior constitution. And precisely similar reasoning, of course, proves that outside the earth's surface there would also be a magnetic field. (*Vide* addition at the end of the paper.) To determine the strength of this field we have the following relationship to start with.

In § 526, Clerk Maxwell's 'Electricity,' it is shown that an element of current C, of length ST, acts upon a unit magnetic pole at a point P with a force

$$C\frac{\mathrm{ST}}{\mathrm{PS}^2}\sin \mathrm{PST},$$

in a direction at right angles to PS and ST. Combining this with the experiments referred to above, we may assume that if a charge of static electricity (measured in electromagnetic units) Q, at the point S, moves in a direction ST with a velocity v *relative* to a point P, it produces on a unit magnetic pole at P a force

$$\frac{Qv}{\mathrm{PS}^2}\sin \mathrm{PST},$$

in a direction at right angles to PS and ST; and this is the *only assumption* employed in the following investigation.

Now suppose the earth to have the uniform density of electricity σ over its surface, and let its radius be unity. Consider the force produced, by the rotation of the electricity at a point S on the surface having coordinates r, θ, ϕ, at a point inside the sphere having the coordinates r_1, θ_1, ϕ_1. Then if the sphere be rotating with an angular velocity w round the axis of z, and if θ be the angle between this axis and a radius, while ϕ is the angle between the axis of x and the projection of a radius, the velocity of S relative to P will have for its coordinates

$$u \text{ or } -w(\sin\theta\sin\phi - r\sin\theta_1\sin\phi_1) \text{ parallel to } x,$$

$$t \text{ or } \quad w(\sin\theta\cos\phi - r\sin\theta_1\cos\phi_1) \text{ parallel to } y;$$

also

$$\text{PS}^2 = l^2 + m^2 + n^2,$$

where

$$l = \sin\theta\cos\phi - r\sin\theta_1\cos\phi_1,$$

$$m = \sin\theta\sin\phi - r\sin\theta_1\sin\phi_1,$$

$$n = \cos\theta - r\cos\theta_1.$$

Now the direction-cosines of PS are proportional to l, m, and n; and the direction-cosines of ST, the direction of motion of S *relatively* to P, are proportional to u, v, or to $-wm$, wl, and 0. Consequently PS is perpendicular to ST. Also, if λ, μ, ν are the direction-cosines of a line at right angles to PS and ST,

$$\lambda = \frac{nl}{\text{PS}\sqrt{l^2+m^2}},$$

$$\mu = \frac{mn}{\text{PS}\sqrt{l^2+m^2}},$$

$$\nu = \frac{-(m^2+l^2)}{\text{PS}\sqrt{l^2+m^2}},$$

where the negative sign must be given to the root. Now if F is the force at P due to the charge of surface-density σ on the elementary area $d\theta \,.\, \sin\theta \,.\, d\phi$ at S moving relatively to P, we know that it must be at right angles to PS and ST, and equal to

$$\frac{\sigma\sin\theta \,.\, d\theta \,.\, d\phi \times \sin \text{PST}\sqrt{u^2+t^2}}{\text{PS}^2}.$$

Hence, if δX, δY, δZ be the resolved portions of the force F parallel to the axes of coordinates, and if dS stands or $\sin\theta \, . \, d\theta \, . \, d\phi$, we have

$$\delta X = \frac{\sigma \, . \, dS \, . \sqrt{u^2+t^2}}{PS^2} \lambda ;$$

and similarly for δY, δZ.

Hence, if $\iint$ stands for the summation over the whole surface of the sphere, and if X is the total force at P in the direction of the axis of x,

$$X = -\iint \frac{\sigma \, dS \, w\sqrt{l^2+m^2}}{PS^2} \frac{nl}{PS\sqrt{l^2+m^2}}$$

$$= -\iint \frac{\sigma w \, dS}{PS^2} n \frac{l}{PS}.$$

Similarly,

$$Y = -\iint \frac{\sigma w \, dS}{PS^2} n \frac{m}{PS}$$

$$Z = \iint \frac{\sigma w \, dS}{PS^2} \frac{m^2+l^2}{n} \frac{n}{PS}.$$

Now the resolved part of the force along the axis of x is the same as would be the force in that direction due to a distribution of attracting matter of density $-n\sigma w$ over the surface of the sphere. Similarly the resolved part of the force along the axis of y is the same as would be the force in that direction due to a distribution of attracting matter of density $-n\sigma w$ over the surface of the sphere; and the force in the direction z is the same as would be the force in the same direction due to a distribution having a density $\sigma w \frac{l^2+m^2}{n}$ or $\sigma w \left(\frac{PS^2}{n} - n\right)$. Regarding the force as being due to such a distribution,

$$Z = \iint \frac{\sigma w \, dS}{PS^2} \, . \, \frac{PS^2}{n} \, . \, \frac{n}{PS} - \iint \frac{\sigma w n \, dS}{PS^2} \, . \, \frac{n}{PS}.$$

Now the first integral is $\iint \frac{\sigma w \, dS}{PS}$, which we know has a value equal to the potential inside the sphere due to a uniform distribution of density σw over the surface, and is therefore a constant, $4\pi\sigma w$. So that the entire force in the direction z is $4\pi\sigma w$ minus the force in direction z due to a distribution of attracting matter of density $n\sigma w$ over the surface of the sphere.

Now it is easy to show that a distribution of attracting matter of a density proportional to n or to $A+C\cos\theta$ over the surface of a sphere will give

$$X=0,$$
$$Y=0,$$
$$Z=\text{ a constant};$$

therefore all that is necessary is to determine the value of this constant. We neglect the term A, because a uniform distribution produces a constant potential, or a zero force in all directions; the distribution $C\cos\theta$ being a zonal harmonic, produces a potential inside the earth,

$$V=\frac{4\pi C}{3}r\cos\theta_1,$$
$$=\frac{4\pi}{3}Cz;$$

so that the force Z which equals $\frac{dV}{dz}$ is $\frac{4\pi}{3}C$. Thus for the distribution $C\cos\theta$ we have the force $\frac{4\pi}{3}C$; so that the constant force above mentioned, $4\pi\sigma w$, requires the distribution $3\sigma w\cos\theta$. From this we must subtract the distribution $n\sigma w$, or $\sigma w(\cos\theta-r\cos\theta_1)$, giving us for the total distribution of attracting matter over the surface of the sphere a density

$$2\sigma w\cos\theta+\sigma wr\cos\theta_1;$$

but the latter term means a uniform distribution, producing therefore no internal force, and may therefore be neglected. And the first term is a zonal spherical surface harmonic; therefore the electromagnetic potential due to the rotation of the electricity on the surface of the earth is

$$\frac{4\pi}{3}2\sigma wr\cos\theta_1 \text{ inside the earth,}$$

and

$$\frac{4\pi}{3}2\sigma w\frac{1}{r^2}\cos\theta_1 \text{ outside the earth,}$$

where w is the angular velocity of the earth on its axis, r the radial distance of any point from the earth's centre, θ_1 the colatitude of the place, $4\pi\sigma$ the total quantity of electricity uniformly distributed over the surface of the earth measured electromagnetically, and the unit of length the earth's radius.

These results, which we think are logical consequences of the experiment performed in Professor Helmholtz's laboratory, and referred to at the commencement of this paper, may now be applied in various ways.

For example, if the iron of the earth is arranged nearly in a hollow sphere, of external and internal radius a_2 and a_1, then, since the potential given above is a zonal harmonic, we can at once apply Poisson's result; and we find that the potential due to magnetization of the hollow sphere is

$$\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)}\frac{\cos\theta}{r^2}$$

for all points outside the outer surface of the sphere; and hence, for points outside the surface of the earth, the total magnetic potential is

$$V=\left[\frac{4\pi\kappa(3+8\pi\kappa)(a_2^3-a_1^3)\frac{8\pi}{3}\sigma w a_2^3}{9+36\pi\kappa+32\pi^2\kappa^2(a_2^3-a_1^3)}+\frac{8\pi\sigma w}{3}\right]\frac{\cos\theta}{r^2},$$

where κ is the coefficient of magnetization.

Now Biot's approximation to the law of intensity of the force is

$$\sqrt{1+3\sin^2\lambda},$$

where λ is the latitude of the place; and we understand that this approximation is generally considered, for rough purposes, as a fairly accurate one.

Our equation for any point at a distance r from the centre and having a colatitude θ is

$$V=M\frac{\cos\theta}{r^2};$$

and

$-\frac{dV}{d\theta}$ is the force directed towards the north,

$-\frac{dV}{dr}$ is the force directed downwards towards the earth's centre;

therefore, if I be the magnetic intensity,

$$I^2=\left(\frac{dV}{d\theta}\right)^2+\left(\frac{dV}{dr}\right)^2$$

$$=\frac{M^2\sin^2\theta}{r^4}+\frac{4M^2\cos^2\theta}{r^6},$$

or, putting r equal to unity (that is, for a point on the earth's surface),

$$\begin{aligned} I &= M\sqrt{1+3\cos^2\theta} \\ &= M\sqrt{1+3\sin^2\lambda}, \end{aligned}$$

which is Biot's expression.

Now this is a result which could not have been anticipated, and speaks well for our new theory of terrestrial magnetism.

It is well known that many forms of distribution of iron inside the earth may be found which, with the existing potential given above, will produce Gauss's distribution of potential over the surface of the earth; and it would be very interesting to try how close an approximation to the real potential would be obtained by considering the iron of the earth to form a hollow ellipsoid, one of its principal axes coinciding nearly, but not quite, with the earth's axis. This calculation would be comparatively easy; but we prefer at present merely confining ourselves to simple illustrations of our theory.

Thus, let us, for simplicity, assume that the magnetic matter of the earth is iron, with everywhere a coefficient of magnetization

$$\kappa = 30^*;$$

then the terms involving κ^2 will be large compared with the others; so that if, for a very rough approximation, we assume a_1 to be nought, and a_2 equal to unity, or the whole earth to consist of iron, we find

$$V = \frac{16\pi}{3}\sigma w \frac{\cos\theta}{r^2}.$$

Now Gauss gives for the magnetic moment of the earth,

$$3{\cdot}3092\, n^3,$$

in millimetre-milligramme-second units, and where n is the number of centimetres in the earth's radius. Consequently, since the dimensions of a magnetic moment are

$$M^{\frac{1}{2}} L^{\frac{5}{2}} T^{-1},$$

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* κ for ordinary iron is probably between 20 and 30; and in our ignorance of the internal state of the earth, or what effect the great heat or pressure may have on the coefficient of magnetization, we are compelled to use this value; but it is possible, of course, that the real value of κ may be very different.

the earth's magnetic moment becomes

$$\cdot 00033092\, n^{-\frac{5}{2}}\, n^{3},$$

the units being the earth's radius, gramme, second.

Assuming Biot's distribution of magnetic force over the surface of the earth, which is also what our theory has led us to, we then get from Gauss's expression for the moment the result that the magnetic potential on the earth is

$$0\cdot 33092 \cos\theta\, n^{-\frac{1}{2}},$$

or

$$0\cdot 00001311 \cos\theta \text{ nearly};$$

so that roughly we have, for a point on the earth's surface,

$$\frac{16\pi}{3}\sigma w = 0\cdot 00001311.$$

But

$$w = \frac{2\pi}{24 \times 60 \times 60};$$

∴ the density

$$\sigma = 0\cdot 0107 \text{ unit of electricity},$$

or the total charge

$$= 4\pi \times 0\cdot 0107,$$

the fundamental units of space, mass, time being the radius of the earth, the gramme, and the second. But the dimensions of σ are $M^{\frac{1}{2}} L^{\frac{1}{2}}$; so that, in order to express σ in C.G.S. units, we must multiply by $\sqrt{n}$; therefore the total charge

$$= 4\pi \times 0\cdot 0107 \sqrt{\frac{2{,}000{,}000{,}000}{\pi}} \text{ C.G.S. units}$$

$$= 4\pi \times 0\cdot 0107 \sqrt{\frac{2{,}000{,}000{,}000}{\pi}} \times 10^{7} \text{ microfarads.}$$

To get an idea of the electromotive force required to produce this charge, let us imagine one pole of a Daniell's battery connected with the earth and the other with all bodies in space. Then, since the capacity of the earth is 630 microfarads, this charge will be produced for each cell so employed; so that, if x is the number of cells necessary to produce our required distribution,

$$x = \frac{4\pi \times 0\cdot 0107 \times \sqrt{\frac{2 \times 10^{9}}{\pi}}\, 10^{7}}{630}$$

$$= 54 \text{ million roughly.}$$

We have therefore proved that if the earth be electrified, it must, from its very rotation, quite independently of all other bodies in the universe, be magnetic; and if it consist of a shell of iron, thick or thin, then that the law of distribution of magnetism produced by this electric charge in mechanical rotation will be identically that given by Biot; and, lastly, if the earth were wholly of iron, a difference of potentials of about fifty-four million volts between it and space would be sufficient to produce the necessary amount of charge.

Now, although fifty-four million volts is a large difference of potentials to be produced with a galvanic battery, there would not be the slightest difficulty in the earth having such a difference of potentials between it and space, seeing that the earth is surrounded by millions of miles of interplanetary vacuum, every inch of which is as good or better an insulator than a Crookes's vacuum; and it has been experimentally shown that many thousands of cells will not cause a discharge across even a comparatively thin film of such a vacuum.

But even without considering the highly insulating character of interplanetary space, we see from the experiments of Drs. De La Rue and Hugo Müller that the electromotive force of fifty-four million cells cannot, in all probability, initiate a spark between two points in ordinary air unless the distance separating them be less than four hundred feet. Consequently, if the electric charge which by its mechanical rotation produces the magnetism be on the earth itself and not in the air, it could not be discharged by sparking, unless another planet, having at least a potential nought relatively to the earth, came at least to within four hundred feet of its surface.

Next, as regards the sign of the electric charge on the earth's surface required to produce the earth's magnetic polarity, is it in accordance with the known phenomena of atmospheric electricity? To produce the earth's magnetism, we must have, in accordance with the known laws of electromagnetism, a *negative* current flowing from west to east, or in in the direction of rotation of the earth. In the language of the new theory, therefore, the surface of the earth must be *negatively* charged; but Sir William Thomson has proved, by observations with his electrometer, that *all* the phenomena brought to light by atmospheric electricity, on a fine day, could be produced by the sole agency of the earth having a *negative* charge and without any charge in the air itself. The negative charge, therefore, required for our explanation of the cause of terrestrial magnetism is sufficient to account for all the ordinary phenomena of atmospheric electricity.

In the preceding investigation we have supposed the electric charge to be uniformly distributed over the earth, and so have arrived at a law of magnetic intensity merely varying with the latitude. But the sun and other members of the solar system may very likely have potentials so different from that of the earth that we can hardly conceive the amounts; consequently we should expect the static electric distribution of the earth would undergo periodic changes corresponding in time with those of the ocean-tides. But alteration in the static distribution of electricity on the earth's surface means, as we have shown, alteration in the law of magnetic intensity; consequently we should expect that this magnetic intensity would vary somewhat as do the ocean-tides; and this is known to be the case. But it is also evident that, besides these regular changes, every time a great mass of vapour is suddenly formed and condensed on the earth, and whenever great changes are occurring in the solar atmosphere whereby the lines of *electrostatic induction* from the sun to the earth are altered, we should find corresponding changes in terrestrial magnetism such as we now know as magnetic storms. And not only this, but as the planets are charged bodies, their motions relatively to the sun ought to cause motions in the sun's atmosphere such that, for instance, the allineation of a number of planets and the sun, or the near approach of any planets, if the alligned or approached planets have potentials nearer that of the sun than many of the other bodies of the solar system, ought to diminish the storms in the solar envelope, and ought to alter the electrostatic distribution on the earth. But it has been shown that the near approach of a planet to the sun both affects the sun's spots and terrestrial magnetism.

And, lastly, since the iron in the earth may, from its great pressure, possess great coercive force, we should expect (as we know to be the case) that magnetic changes would lag behind the astronomical influences accompanying them.

Addition, April 17th.—Since the reading of this paper before the Physical Society, several criticisms have appeared of this proposed explanation of terrestrial magnetism. Some of the writers have failed to realize that the various parts of a rotating sphere have relative motions one to the other, of such a nature that if some were electrified the others would become magnetized. Their difficulty seems to have arisen from the fact that the motion of a particle of a rotating rigid sphere consists of a rotation round the axis of the sphere combined with a rotation of the particle round its own axis; so that if two particles be looking at one another in one position of the sphere, they are looking at one another in all positions, just as the same side of the moon is always turned to the earth. But if this rotation of the particles round their own axes could be stopped, if, in fact, the motion of the particles became what is commonly known as "sun-and-planet motion" similar to that of the bobbins in the machinery used in sheathing telegraph-cables to prevent torsional strain being put into the iron wires as they are lapped on the core), then probably these writers would have no difficulty in seeing that the interior particles would be magnetized by the more rapidly moving electrified ones. Now the motion of an *un*electrified particle round its own axis cannot in any way prevent it becoming magnetized by electrified particles revolving round it; for if it could, it would be equivalent to saying that, if the electrified particles were at rest and the *un*electrified one revolving, the mere revolution of the latter would magnetize itself oppositely to the way it would be magnetized if it were at rest and the electrified ones only in motion—a result not only without experimental proof, but one also highly improbable. Consequently, if the particle has the two motions together (as it has in a rigid revolving sphere), it will still become magnetized if the surface of the sphere is electrified.

In fact, so little can the motion of a mass of iron prevent its becoming magnetized by a moving charge of electricity, that it has been suggested to us, within the last few days, by Mr. G. F. Fitzgerald, of Trinity College, Dublin, that probably a mass of iron would become magnetized by a static charge of electricity if both had rapid absolute motion in space, even although in the same direction in parallel lines and with the same velocity—in fact, that *relative* motion is unnecessary. If this assumption (which has not yet been experimentally tested) be true, then not only will the mathematical analysis required in the investigation of the problem contained in this paper be much simplified, but, in addition the charge of electricity on the earth's surface necessary to produce, by its mechanical rotation, the earth's known magnetic moment will be considerably less even than the charge calculated above. We hope to put this idea of Mr. Fitzgerald's shortly to an experimental test; but in the meantime we give no further indication of the reasoning by which it has been arrived at, nor of the way in which our equations would be modified, preferring to leave the investigation in a perfectly rigid form as it now stands, rather than to introduce any assumption which might appear problematical, even although such an introduction would both add weight to our theory, and might explain, from the velocity *in space* of a place at midnight being greater than at midday, the cause of the solar-diurnal magnetic variation.

X. *The Maintenance of Constant Pressures and Temperatures.*

By FREDERICK D. BROWN, *B.Sc.*

[Plate IX.]

THE great majority of the results obtained from physical experiments vary with the temperature at which the observations are made. The measurements of the density of a substance, for example, of its refractive index, of its electric conductivity, of its elasticity, of the maximum tension, and of the latent heat of its vapour, all require that the temperature should not vary during the observations. Hitherto many of these measurements have been confined to temperatures differing little from that of the atmosphere; such temperatures are easily maintained constant by means of a bath of water or other liquid; but when we try to make observations at higher temperatures, the means at our disposal fail us, and we find that, except at certain points (such as 100°), we cannot keep up the same temperature long enough to make at leisure accurate readings of our instruments.

Many attempts have been made to obviate this difficulty, but, as it seems to me, without complete success. The ordinary method has been to use a large quantity of water or other liquid, and to keep it in continual agitation; above 50°, however, the temperature of such a bath is rarely, or never, rigorously constant, while the inconvenience and waste of time incurred in heating the large mass of liquid to the required point are by no means to be neglected.

In order to keep such a bath at a constant temperature, a large number of gas-regulators have been invented. In most of these a small vessel containing mercury or air is placed in the bath, and arrangements are made by which the gas-supply is partially shut off when the mercury expands beyond a certain point. Probably the most sensitive form of this kind of apparatus is that recently described by M. Benoit to the French Physical Society. Here a small closed vessel containing methyl acetate is placed in the water the temperature of which is to be maintained constant; this vessel communicates with a manometer containing mercury; and as the vapour-tension of the methyl acetate increases, the mercury in the manometer rises, obstructing the flow of gas to the burner in the usual way.

I have applied this form of thermostat to a bath of water used for heating a long column of mercury. The water is contained in a vertical copper cylinder 42 inches in length by 6 in diameter; the cylinder is packed with felt on the outside, and contains within it a second smaller and shorter one open at both ends, and extending to within about 2 inches of the top and bottom. By means of a suitable mechanical arrangement, the water is made to flow continuously down the space between the two cylinders and up the inner cylinder; the upper and lower portions of the bath are thus kept at exactly the same temperature. The water is heated by allowing it to circulate through a copper coil placed over a gas-burner. Even when the small vessel of the thermostat is filled with ether, the tension of which varies much more for a given variation of temperature than does that of methyl acetate, the temperature is subject to fluctuations of as much as 0°·1, and this independently of the change which necessarily occurs when the pressure of the gas is altered.

Gas-regulators are employed perhaps more successfully where the gas-flame can be applied directly underneath the bath; for the rise of temperature in the water then follows more rapidly upon the increase of the flame, the flow of gas is sooner checked by the mercury, and the tendency to allow too much heat to be communicated to the water is thereby lessened. Still better results are obtained if the thermostat be applied to an outer jacket of water surrounding the bath in which the observations are made. Both these conditions, however, are generally very difficult to carry out where large quantities of water are of necessity used. Even when all these precautions are taken, a constant temperature, in the true meaning of the words, is not attained. For a further discussion of the defects of this form of thermostat see Laspeyres*.

The only practicable way of attaining the object in view appears to be afforded by the fact that the vapour emitted by a boiling liquid does not vary in temperature, provided that there is no variation either in the composition of the liquid or in the pressure to which it is subjected. Taking their stand on this consideration, Laspeyres (*loc. cit.*) and Sprengel (Journ. Chem. Soc. 1873, p. 458) suggested the use of mixtures of sulphuric acid and water of different strengths, which, if the vapour given off is condensed and returned to the mass, boil constantly at certain given temperatures: here the constant temperature is afforded by the liquid and not by the vapour, which is less hot; hence, if the liquid tends to become superheated, fluctuations in temperature will infallibly occur. The great objection to this method is that, when a series of temperatures is required (as, for instance, in the comparison of thermometers), great inconvenience and loss of time is incurred by having frequently to replace the liquid in the apparatus by another containing a different proportion of sulphuric acid or other substance.

A series of temperatures can be obtained with one liquid only, if the pressure under which it boils be varied. This method, simple as it appears, is beset with considerable mechanical difficulties, to surmount which the apparatus described below has been constructed.

At first sight it would seem that, if the vessel containing the steam be connected with a large closed vessel serving as air-reservoir, all that is necessary is to rarefy or compress the air in the reservoir to the required extent, and to allow the liquid to boil undisturbed. But the steam-bath is necessarily large; and the reservoir therefore must be large also. Now the reservoir must not only be strong enough to stand a vacuum inside it, but must also be capable of supporting an interior pressure of at least 50 lbs. on the square inch; such a reservoir is not only cumbrous and expensive, but dangerous. Further, any change of temperature in the room alters the pressure inside this reservoir; it must therefore be placed in a cistern of water, which is a second objection. The great obstacle to the employment of this method, however, is the difficulty of preventing leakage even with the greatest possible care. In endeavouring to stop leaks I expended so much time that I gave up the reservoir, and determined to construct an apparatus for maintaining a constant pressure in a given vessel even when it leaked.

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* Pogg. *Ann.* clii. p. 132.

Lothar Meyer (*Ann. Chem. Pharm.* vol. clxv. p. 303) had already devised an apparatus of this kind, adapted chiefly to fractional distillation under reduced pressure. It consists essentially of two vertical tubes, A B and C D (fig. 1, Pl. XIII.), connected at the upper part by the lateral tubes E and F; at B an india-rubber tube connects A B with the tube K, which slides up and down the board to which the whole is fixed; at the top of A B is a tube H which is connected with an air-pump; the top of C D is provided with a cork, through which a narrow tube S S passes nearly to the bottom of C D; finally, the lateral tube *k* is connected with the apparatus X, in which a constant pressure is kept up. Sufficient mercury is poured into R to fill A B up to the lower end of H when R is about half full; C D is also filled with mercury, which is let out by the tap M until the column P S above the lower end of S S is equal to the difference between the required pressure and that of the atmosphere. If now the air-pump be set to work, a partial vacuum will be created in X and in the tubes A B and C D, the mercury will rise in A B until it touches the lower end of H; R is then placed in such a position that the vertical distance between the lower end of H and the surface of the mercury in R is equal to P S. It is now evident that when the desired pressure is reached the mercury will close up the orifice of H, thus stopping the withdrawal of air; while if the pressure is less than that required, more air will enter through the tube S S and bubble up through the mercury, and thus a more or less constant pressure will be maintained in X.

This apparatus suffers from two defects: first, the splashing of the mercury as it is sucked up H and then falls down again, together with the bubbling of air up C D, renders the pressure in X slightly variable; secondly, it is only adapted for pressures less than that of the atmosphere.

Meyer's instrument has very recently been modified by Dr. Otto Schumann and by W. Stadel and E. Hahn (*Ann. Chem. Pharm.* vol. cxcv. p. 218); the new form, although capable of regulating pressures above an atmosphere, apparently with tolerable accuracy, has a very limited range.

The apparatus for maintaining constant pressures, which I now wish to bring to the notice of the Society, consists of a manometer connected with which is an automatic arrangement for governing the supply of air. The manometer (fig. 2) consists of a tube A B having the form and dimensions shown in the figure; the lateral tube C is connected with the vessel X, in which a constant pressure is to be kept up; the tube D is connected with an air-pump or other contrivance for rarefying and compressing air. The upper end of the tube A B is closed by an india-rubber stopper, or, better, by a metal cap, through which the rod E_1 passes air-tight; this rod is tipped with platinum at its lower end. The lower end of A B is joined by means of india-rubber tube to the tube F, of which the upper part has the same diameter as that of A B; this tube is fitted with another cap and iron rod E_2 similar to the first; but the cap does not fit air-tight. The piece of wood which carries F moves along the scale S S in a groove in the board to which the whole is fixed. Lastly, the two rods E_1, E_2 are furnished with binding-screws for copper wire, while a third binding-screw K is connected with a small piece of steel tube interposed between the end of A B and the india-rubber tube. This third connexion with the mercury contained in the manometer may of course be made by means of a third (insulated) rod passing down to the bottom of the wider part of A B or F.

To fill and adjust the manometer, the tube F is raised to the same level as A, and mercury is poured in until it just reaches the wide cylindrical portions of A and F; the rods E_1, E_2 are then moved until the point of E_1 just touches the surface of the mercury, while that of E_2 is a fraction of a millimetre above it.

Suppose now that F be lowered n millims. and the air-pump be set to work to pump air out at D; further, that K be connected with one pole P of a battery, and the rods E_1, E_2 with the other pole N; then it is evident that as long as the pressure in A, and consequently in X, is greater than $H-n$ (where $H=$ the barometric pressure), the current will pass through the circuit P K E_2 N, whereas when the pressure is less than $H-n$ the current will pass along P K E_1 N. Similarly, if F be raised n millims. above A and the pump be made to compress air into A, as long as the pressure is less than $H+n$, the current will pass along P K E_1 N; but when the pressure becomes greater than $H+n$, the current will pass along P K E_2 N. In order to economize space, the tube A B is widened out at B; and when it is intended to obtain a pressure greater than H, the rod E_1 is replaced by a long one reaching down to B; this, of course, is equivalent to lowering A or raising F nearly the whole length of the scale S S. In the further description I shall only consider the case in which F is lower than A—that is, when a pressure less than H is required; the alteration needed for higher pressures will readily suggest itself.

The apparatus depicted in fig. 3 consists of a brass tap T and an electromagnetic clutch to work the tap automatically. The tap, which is shown in section in fig. 4, is placed between the tube D of the manometer (fig. 2) and the air-pump, L_1 being connected with D and L_2 with the pump. From the figure it is readily seen that when the tap is in the position drawn, and the lever fitted to the head of the key lies in the direction $a\,b$, air will be admitted into the manometer; if, on the other hand, the lever and key occupy the position $a'\,b'$, the pump will withdraw air from the manometer. The object of the clutch, therefore, is to place the tap in the first position when the pressure is too small, and in the second when it is too great. The lever $a\,b$ (fig. 3) terminates in two arcs which are grooved to hold a cord; these arcs are furnished with set screws S_1, S_2, by means of which the amount which the tap can open may be regulated. The two ends of the lever are connected by strings at S_1 S_2 to the loose pulleys P_1 P_2 of the clutch. These pulleys are made of soft iron, and run on a spindle which revolves on the centres A_1, A_2. Revolving with the spindle and facing P_1 and P_2 are two small electromagnets M_1, M_2: one end of the coils of each of these magnets is soldered to the insulated ring I_2; the two other ends are soldered to I_1 and I_3 respectively. The binding-screws U_1, U_2, U_3 are connected electrically by means of springs with these rings; U_2 is further connected with the pole N of the battery, while from U_1 and U_3 wires run to E_2 and E_1 respectively. The disposition of the wires is shown in fig. 5: it is there evident that if the mercury in the manometer touches the rod E_1, M_2 will become magnetic; the loose pulley P_2 (fig. 3) will then tend to revolve with it, and the tap will take up the required position *a b*; while if the mercury touches E_2, M_1 will become magnetic, P_3 will revolve, and the tap will assume the position *a′ b′*, in which communication is made with the air-pump. By this arrangement, therefore, the pressure in the manometer, and in whatever apparatus is connected with it, is caused to rise and fall within very small limits; with care these limits may be made to differ only about 0·25 millim.; and thus a practically constant pressure is attained.

The current required to work the magnets is no more than is furnished by a small Smee's cell; the magnets may be made to revolve with a small turbine; the air-pump may be replaced by a Bunsen water-pump; an air-pump, however, is the only convenient apparatus for *compressing* air.

It may be objected that the arrangement above described requires motive power, which is not always at hand in a laboratory. To meet this objection I endeavoured to construct a double valve to be moved to and fro by two stationary magnets; but I found that magnets of ordinary size were not powerful enough for the work, as the valve, to be of any use, must fit perfectly air-tight, even when subjected to very high pressures. I have not, however, given much attention to this point, an engine which I use for many other purposes being obviously the best source of the necessary power.

In fig. 6 is given a section of the steam- or vapour-bath which I employ for the comparison of thermometers and for the measurement of the expansion of liquids in dilatometers; it is also suitable for the direct comparison of thermometers with a standard air-thermometer. The bath is made of brass, and consists merely of a boiler B surmounted by a double tube D,D similar to those first used by Rudberg and Regnault for the upper fixed point of thermometers; the vapour, after traversing this, passes into the U-shaped condenser C, from which the condensed liquid runs back into B down the tube E. The end H of this condenser is connected with the lateral tube C of the manometer by means of a very small lead pipe; and thus the liquid can be made to boil under any required pressure. The thermometers or other instruments are placed in the small tubes T T, which are filled with petroleum of high boiling-point. Besides water, the best liquids for generating the vapour appear to be carbon disulphide for low, and purified paraffin oil for high temperatures; the latter substance I have not hitherto used, as I have had no occasion to make observations at such temperatures. I have, however, made experiments with it in a smaller apparatus of similar form, and found that no variation of temperature took place.

The temperature obtained from these three liquids may be varied from 25° to 300° without unduly increasing the pressure.

If the temperature in the double tube D D be observed with a thermometer of which each degree is 5 millims. long, no variation can be detected, even with an extremely rigid cathetometer, provided, of course, that the barometric pressure does not alter; the variation due to this cause might, if necessary, be removed by making the cap of the tube F of the manometer fit air-tight. When great accuracy is necessary, it is not advisable to decrease the pressure below 100 millims.

In order to see whether the whole length of D D is at the same temperature, small oblique tubes similar to T were inserted, one at the top, the other at the bottom; a thermometer placed first in one and then in the other of these, gave exactly the same reading in both.

To sum up. First, with the above apparatus, viz. a manometer communicating by means of a specially constructed tap worked by a double electromagnetic clutch with a constantly working air-pump, any given pressure may be maintained for an indefinite period without varying more than 0·25 millim.; secondly, if this constant pressure be applied to a suitable vapour-bath, any given temperature between 25° and 300° may be maintained absolutely constant as long as no chemical change occurs in the liquid whence the vapour is derived.

XI. *On the Fracture of Colloids.* By Frederick Guthrie*.

[Plates X. & XI.]

§ 1. A prominent property with regard to solid colloids is that they have neither crystalline form nor planes of cleavage. When such a body is broken it offers the so-called conchoidal fracture. An agglomeration of crystals may present in mass the conchoidal fracture usually associated with colloids. This is the case with granite, and eminently so with basalt, all of whose constituents are crystalline. When the solid has resulted from the intersolution of two or more crystalloids it may, like glass, present the colloidal fracture in a most marked manner. And, indeed, even single crystals themselves are often subcolloidal in fracture; that is, conchoidal fracture accompanies the crystalline. This state is shown by the diamond, sugar-candy, quartz, &c.

I assume here that every cohesionally homogeneous mass of solid matter will break conchoidally when subjected to pressure sufficient to cause fracture.

Experiments.

§ 2. The cracking of a glass plate by pressure offers no special features of interest. A round plate placed on a thick soft cloth and pressed in the centre by a round cork cracks radially; the cracks are generally slightly curved. Fig. 1 shows two examples of fracture of crown glass by pressure in the centre. Similarly, if a round sheet of glass placed on a thick soft cloth be pressed down at its circumference by means of cardboard rings, the same class of crack is produced; for, indeed, the two conditions are essentially identical.

§ 3. The internal strain caused by difference of temperature causes fracture of great regularity and beauty. It rarely happens that a sheet of glass of any shape breaks into only two pieces when heated. If a circular piece of "crown" glass, about 3½ inches in diameter, be loosely balanced horizontally between the lips of a wooden clip and brought with its centre over an air-gas burner so that the top of the flame is about an inch below the glass, the latter almost invariably cracks at least into three pieces ; and when the pieces are three in number they as invariably have the form shown in fig. 2.

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* Read March 22, 1879.

The remarkable symmetry of each of these, and their similarity to one another, show that the shape is not an accident of the glass. The constant features are (1) that the two main cracks join before reaching the circumference, (2) that there are in each crack, reckoned from this confluence, three concavities towards the centre of the circle, the first being nearly straight, (3) that there is a little kick given by the crack as it leaves.

Out of sixty-four specimens of fracture produced under these conditions ten showed this species of two-crack fracture. The shapes of the cracks are perfectly similar to those given; and the symmetry is sometimes such that the side pieces may replace one another after inversion so perfectly that it is scarcely possible to tell that they are misplaced.

§ 4. The same method of heating may result in the production of a great variety of forms ; but they are all derived from the above type. In fig. 3 are shown a few of the more simple.

The three-crack figures (*a*, *a*) are about as frequently formed as the two-crack figures. I find eight of the three-crack out of sixty-four similarly treated specimens. Perfectly similar forms were got when the plate was laid on a retort-ring or supported on three corks and heated in the same manner. A special series of experiments was moreover made to see if the position of the clip had any influence upon the attitude of the crack. In figures 2 and 3 the mark † shows where the plate was held. As to the effect of the position in the original sheet of glass of the pieces experimented on, as determining the attitude of the axis of cracking, the following examination was made. Six pieces having been marked as they lay in the sheet, forming a radial band, were cut out and heated as above. The apex of crackage always appeared somewhere on the semicircle which was towards the centre of the sheet, but varied in this semicircle so considerably that it is at present doubtful whether the position in the sheet influences the crack-axis. Fig. 4 shows the amount of variation; the point of the arrow represents the apex of the crack-curve. The lower figure represents the original sheet and the positions of the several pieces in it.

§ 5. If the sheet of glass be made very much larger, or the flame smaller and more pointed, another alteration of the crack-figure ensues. The apical point of the previous figures advances into the sheet; and this is followed by a fan-like cracking of the glass between the apex and the still nearest circumference. In fig. 4, *a* shows the cracking of a plate of crown glass, 9 inches in diameter, over an air-gas burner; *b* is a 5½-inch-diameter plate similarly treated. In *c* we have a plate of crown glass, 3 inches in diameter, which was laid on a cloth and heated from above by a fine blowpipe-flame. If we conceive what was before called the apical point to reach the centre, the heat fracture would become approximately the central-pressure fracture, namely radial.

§ 6. A piece of plate-glass ¼ inch thick and a little over 7 inches in diameter, cracked when heated in the centre over an air-gas burner, as shown in fig. 5 *a*. A piece of "sheet-glass" (Chance's), 3 inches in diameter, cracked as shown in *b*. A slab of resin ½ inch in thickness and 3½ inches in diameter, heated in the centre by a jet of low-pressure steam, gave the fissures shown in *c*. Square porcelain tiles cracked nearly straight across in one crack.

§ 7. Pieces of crown glass of various shape were next examined, with the result which declares itself in fig. 6. The pieces were supported at the point marked *c*, and the flame applied below the point marked †.

The figure 6 shows that the same general type is preserved. It instructs us that the apical point seeks one of the nearest points of the circumference.

§ 8. Experiments were next made for the purpose of ascertaining under what circumstances, if at all, a crack could cross a crack. A circular plate of crown glass was cut by the diamond in concentric rings, and the crack was made to pass, by tapping, completely through the thickness of the glass, around the whole circumference. Such divided glass on being heated in the centre over an air-gas burner cracked according to the same type as before. Sometimes the heat-crack would run across the diamond-crack, as though the latter had no existence. Sometimes the heat-crack would follow, and, as it were, adopt the diamond-crack, and then break off. In the latter of such cases the inner circle may be suffering a three-crack fracture, while the outer ring exhibits only a two-crack fracture on the converse. Fig. 7 (*a*) exhibits the former circumstance, fig. 7 (*b*) shows the influence of a greater number of concentric cracks.

§ 9. The heating of the central part of a circular plate should give the same crack-figure as the cooling of the circumference; and, as a matter of experiment, it is found that the figures are very similar. In order to cool the circumference of a heated circular plate with some approach to uniformity, an annular trough was constructed by cementing concentrically two glass cylindrical vessels of the same depth but different diameters, one inside the other, and over-filling with mercury, so that the convex surface of the metal projected. The glass was held in a wooden clip widely stretched, so that the axis of the clip being vertical the plate was horizontal. Held, when uniformly hot, immediately above the mercury, it was let drop and pressed down in the middle by a piece of wood. The fracture is in this case instructive; for while in fig. 8 (*a*) the old type got by heating the centre is resumed in *b* and *c*, the fracture is either influenced or even accompanied by the circular fracture along or near the line of greatest temperature-difference.

§ 10. It is clear that heating in the central regions should produce a similar fracture to that brought about by cooling around the circumference, and cooling at the centre a similar fracture to that caused by heating the circumference.

On heating a circular plate at the circumference by means of a "rose" air-gas burner, it breaks with far greater violence than when fracture is produced by central heating. The parts are scattered at least three times as far in the former as in the latter case. The form of the fracture is essentially radial; but the fragments, even when the primitive type is widely departed from, present wonderful symmetry. A noticeable point in this fashion of fracture is the invariable appearance of two pieces on opposite sides of the centre whose form is approximately rectangular; that is, their sides are half-cords instead of radii. This form suggests that there are two chief centres of maximum fracture, and that the bounding radii of the two systems are parallel. In fig. 9 the pieces marked *a* represent these singular pieces. Out of seven plates which have been broken in this way, there is not one in which this feature is absent.

§ 11. As to cooling a hot plate in the centre, I find such extreme difficulty in reproducing the inverse conditions of heating a hot plate at the circumference that I have rarely succeeded in reproducing the same type of fracture.

Also it is seldom the case that a sheet of glass cracks during heating at its edge. More frequently a sheet of glass which has been heated at its edge cracks when cooling. The crack then appears to follow that isothermal line along which there is the greatest difference of temperature at right angles to the line.

Remarks.

§ 12. About the fracture by mechanical strain it may appear to satisfy many that the lines of fracture are perpendicular to that resultant of the pressure which lies in the plane fractured. A tear in a sheet of paper is at right angles to the two opposing pressures, or rather to their resultants at the point yielding.

What is a crack? Which are its beginning and end? In only one of the above-cited experiments can the growth of a crack be followed. In § 11 when a plate heated at the edge has refused to crack while being heated but cracks on cooling, the crack is seen to extend from the edge of the plate inwards, following, generally speaking, a semicircular path, but sometimes curiously modified towards the centre of the curve.

A crack is neither a line of least cohesion nor a line of greatest strain. Nor is it a line where $\frac{c}{s}$ has a series of minimum values. The more perfectly elastic a medium is, the more fully does the crack resemble a flash of lightning or wisely laid railway-line, and the more it departs from the river-course or the descent of a globule of mercury down an inclined but undulating surface. Its path is the curve whose course is determined by the integnal of $\frac{c}{s}$ being a minimum. The sudden splitting through of the solid æther by the electric discharge furnishes us with figures by no means remotely resembling those of the fracture of glass. Even or rather especially the forms of fig. 4 remind us of this.

As to the typical form in fig. 2, it has been suggested by my brother, Mr. Charles Guthrie, that this form is a compromise between the circular line of fracture along some isothermal line where the difference of temperature is greatest, or rather the difference of expansion is greatest, with the three lines of relief which would be radii at angles of 120° with one another.

This is a very suggestive hint; but, for reasons sufficiently apparent from the foregoing, it is insufficient.

XII. *Induction-balance and Experimental Researches therewith.*

By Prof. D. E. Hughes.

[Plate XII.]

Immediately upon the announcement of Arago's discovery of the influence of rotating plates of metal upon a magnetic needle (1824), and Faraday's important discovery of voltaic and magneto-induction (1831), it became evident that the induced currents circulating in a metallic mass might be so acted upon either by voltaic or induced currents circulating in a metallic mass as to bring some new light to bear on the molecular construction of metallic bodies.

The question was particularly studied by Babbage, Sir John Herschell, and M. Dove, who constructed an induction-balance, wherein two separate induction-coils, each having its primary and secondary coils, were joined together in such a manner that the induced current in one coil was made to neutralize the induced current in the opposite coil, thus forming an induction-balance, to which he gave the name of "differential inductors." In those days physicists did not possess the exquisitely sensitive galvanometers and other means of research that we possess today; but sufficiently important results were obtained to prove that a vast field of research would be opened if a perfect induction-balance could be found, together with a means of correctly estimating the results obtained. In experimenting with the microphone I had ample occasion to appreciate the exquisite sensitiveness of the telephone to minute induced currents. This led me to study the question of induction by aid of the telephone and microphone: the results of those researches have already been published. Continuing this line of inquiry, I thought I might again attempt to investigate the molecular construction of metals and alloys; and with this object I have obtained, after numerous comparative failures, a perfect induction-balance which is not only exquisitely sensitive and exact, but allows us to obtain direct comparative measures of the force or disturbances produced by the introduction of any metal or conductor.

The instrument now submitted to the Physical Society consists, 1st, of the new induction-currents balance; 2nd, a microphone, with a clock as a source of sound; 3rd, electric sonometer, or absolute sound-measurer, a late invention of my own; 4th, a receiving telephone and three elements of Daniell's battery. In order to have a perfect induction-currents balance suitable for physical research, all its coils as well as the size and amount of wire should be equal. The primary coils *a*, *a′*, and the secondary coils *b*, *b′*, should be separated and not superposed. The exterior diameter of the coils is 5½ centims., having an interior vacant circular space of 3 centims.; the depth of this flat coil or spool is 10 millims. Upon this box-wood spool are wound 100 metres of No. 32 silk-covered copper wire. I use four of such coils, formed into two pairs, the secondary coil being fixed permanently, or by means of an adjustable slide, at a distance of 5 millims. from its primary; on the second similar pair there is a fine micrometer-screw, allowing me to adjust the balance to the degree of perfection required. These two pairs of coils should be placed at a distance not less than one metre from each other, so that no disturbing cause may exist from their proximity. The two primary coils are joined in series to the battery, the circuit also passing through the microphone. In place of the telephone I have sometimes used a magnetic pendulum, the swing or the arc described indicating and measuring the forces. I am at present engaged upon a very sensitive voltameter, which shall indicate and measure the force of rigid induced currents. The telephone, however, is well adapted as an indicator, but not as a measure of the forces brought into action. For this reason I have joined to this instrument an instrument to which I have given the name of electric sonometer (Pl. V.) This consists of three coils *c*, *d*, *e*, similar to those already described, two of which are placed horizontally at a fixed distance of about 40 centims. apart; and the communication with battery is so arranged that there are similar but opposing poles in each coil; between these two there is a coil, *d*, which can be moved on a marked sliding scale, *f*, divided into millimetres, in a line with these two opposing primary coils*. This central coil is the secondary one, and connected by means of a switching key with the telephone in place of the induction-balances. If this secondary coil is near either primary coil, we hear loud tones, due to its proximity. The same effect takes place if the secondary coil is near the opposing coil, except that the induced current is now in a contrary direction, as a similar pole of the primary acts now on the opposite side of the induction-coil. The consequence is, that as we withdraw it from one coil and approach the other, we must pass a line of absolute zero, where no current whatever can be induced, owing to the absolute equal forces acting equally on both sides of the induction-coil. This point is in the exact centre between the two coils.

We thus possess a sonometer having an absolute zero of sound: each degree that it is moved is accompanied by its relative degrees of increase; and this measure may be expressed in the degrees of the millimetres passed through, or by the square of the distances in accordance with the curve of electro-magnetic action. If we place in the coils of the induction-balance a piece of metal (say copper, bismuth, or iron), we at once produce a disturbance of the balance, and it will give out sounds more or less intense on the telephone according to the mass, or if of similar sizes, according to the molecular structure of the metal. The volume and intensity of sound is invariably the same for a similar metal. If by means of the switching key the telephone is instantly transferred to the sonometer, and if its coil be at zero, we hear sounds when the key is up, or in connexion with the wire *g*, which leads to the induction-balance, and no sounds, or silence, when the key is down or in contact with the wire *h*, in connexion with the sonometer. If the sonometer-coil were moved through several degrees, or through more than the required amount, we should find that the sounds increase when the key is depressed; but when the coil is moved to a degree where there is absolute equality, if key is up or down, then the degree on scale should give the true value of the disturbance produced in the induction-balance; and this is so exact that if we put, say, a silver coin whose value is 115°, no other degree will produce equality. Once knowing, therefore, the value of any metal or alloy, it is not necessary to know in advance what the metal is; for if its equality is 115°, it is silver coin; if 52, iron; if 40, lead; if 10, bismuth; and as there is a very wide limit between each metal, the reading of the value of each is very rapid, a few seconds sufficing to give the exact sound-value of any metal or alloy.

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* If the coils *c* and *e* are of unequal size, the zero of the scale will occupy a position similar to that shown on the Plate.

The respective values of the different metals may, as I have already pointed out, be indicated by introducing the sonometer into circuit. I find, however, that it is difficult to estimate fractional differences when the sounds to be compared are loud. I therefore prefer to *balance* the metal under examination by means of a similar mass placed in the opposing coil, reading on the sonometer the differences of sound, which are then slight. Experience has shown that the most accurate results are obtained when the sonometer is replaced by a graduated strip of zinc about 23 millims. wide, 200 millims. long, and tapering from a thickness of 4 millims. at one end to a fine edge at the other, and superposed in a horizontal plane over the opposing coil *b′*, the metal to be tested being in a plane midway between *a* and *b* on the left of the plate.

The delicacy of the readings may of course be greatly increased by diminishing the angle between the two faces of the strip; but there are many points connected with its use which would be too long to describe in this paper.

As a rule three Daniell elements will be found quite sufficient; and even this weak force is so exquisitely sensitive that it will find out the smallest fraction of difference in weight or structure of metals. Thus two silver coins such as a shilling, both quite new, and both apparently of the same weight, will be found to possess a difference of weight which the instrument at once indicates.

The following experiments will show its exceeding sensitiveness and its wide field of usefulness as an instrument of research.

I. If we introduce into one pair of the induction-coils any conducting body, such as silver, copper, iron, &c., there are set up in these bodies electric currents which react both upon the primary and secondary coils, producing extra currents whose force will be proportional to the mass and to its specific conducting-power. A miligramme of copper or a fine iron wire, finer than the human hair, can be loudly heard and appreciated by direct measurement, and its exact value ascertained. We can thus weigh to an almost infinitesimal degree the mass of the metal under examination: for instance, if we take two English shilling pieces fresh from the Mint, and if they are absolutely identical in form, weight, and material, they will be completely balanced by placing one each in the two separate coils, provided that for these experiments there is an adjustable resting-place in each pair of coils, so that each coin may lie exactly in the centre of the vacant space between the primary and secondary coils. If, however, these shillings are in the slightest degree worn, or have a different temperature, we at once perceive this difference, and, if desired, measure it by the sonometer, or by lifting the supposed heaviest coin at a slight distance from the fixed centre line: the amount of degrees that the heaviest coin is withdrawn will show its relative mass or weight as compared with the lightest. I have thus been able to appreciate the difference caused by simply rubbing the shilling between the fingers, or the difference of temperature by simply breathing near the coils; and in order to reduce this sensibility within reasonable limits, I have only used in the following experiments 100 metres of copper wire to each coil, and 3 cells of battery.

II. The comparative disturbing value of disks of different metals, all of the same size and form of an English shilling, and measured in millimetre-degrees, by the sonometer, is the following:—

Silver (chemically pure)	125
Gold " "	117
Silver (coin)	115
Aluminium	112
Copper	100
Zinc	80
Bronze	76
Tin	74
Iron (ordinary)	52
German silver	50
Iron (chemically pure)	45
Copper (antimony alloy)	40
Lead	38
Antimony	35
Mercury	30
Sulphur (iron alloy)	20
Bismuth	10
Zinc (antimony alloy)	6
Spongy gold (pure)	3
Carbon (gas)	2

III. It will be seen from the above that the instrument gives very different values for different metals or alloys ; consequently we cannot obtain a balance by employing two disks of different metals, and the instrument is so sensitive to any variation in mass or matter that it instantly detects the difference by clear loud tones on the telephone. If I place two gold sovereigns of equal weight and value, one in each coil, there is complete silence, indicating identity or equality between them ; but if one of them is a false sovereign, or even gold of a different alloy, the fact is instantly detected by the electrical balance being disturbed. The instrument thus becomes a rapid and perfect coin-detector, and can test any alloy, giving instantly its electrical value. The exceeding sensitiveness of this electrical test I shall demonstrate by experiment, now. Again, as regards coins, it resolves an almost magical problem. Thus, if a person puts one or several coins into one pair of coils, the amount or nominal value being unknown to myself, I have only to introduce into the opposite coils different coins successively, as I should weights in a scale, and when perfect balance is announced by the silence, the amount in one box will not only be the same nominal value but of the same kind of coin.

IV. We find, by direct experiment with this instrument, that the preceding results are due to electric currents induced by the primary coil, and that it is by the reaction of these that the balance is destroyed; for if we take an insulated spiral disk or helix of copper wire with its terminal wires open, there is no disturbance of the balance whatever, notwithstanding that we have introduced a comparatively large amount of copper wire; but on closing the circuit the balance is at once very powerfully disturbed.

If the spiral is a flat one, resembling a disk of metal, and circuit closed, we find that loud tones result when the spiral is placed flat, or when its wire is parallel to those on the coils; but if it is held at right angles to these wires, no sound whatever is heard, and the balance remains perfect. The same thing occurs with disks of all non-magnetic metals, and a disk of metal placed perpendicular to the coils exerts no influence whatever. The contrary result takes place with a spiral of iron wire or disk of iron: the induced current circulating in the spiral is at its maximum when the spiral lies flat or parallel with the coils, giving no induced current whatever when at right angles; but the disturbances of the induction-balance are more than fourfold when perpendicular to the wires of the coils than when parallel with the same. This result is simply due to the property of magnetic bodies, of conduction of magnetism.

V. If we introduce a disk of metal gradually into the coils, we find that its power increases as the square of the distance, until it arrives at its maximum exactly in the centre of the vacant space between each pair of coils, diminishing rapidly in the same ratio if the disk be moved towards primary or secondary. Thus, in the interior of the coils there is but a single line of maximum force; but at the exterior we have on each coil two maxima and one minimum, the first and most powerful maximum being in the centre of vacant space between each pair of coils, coinciding with the maximum lines of force of the centre of the coils; the minimum lines of force are exactly in the exterior centre of each coil, again rapidly rising to a maximum near the exterior edge of coils, and gradually diminishing in power from this point.

If we place exteriorly a bar of metal in the centre line of vacant space, we find that it has here its maximum disturbing power, giving out loud sounds.

If we now move this bar until it rests on the centre of either coil, we find that at this point the bar has no disturbing effect whatever, and although the coils at its maximum line of force are sensitive to the finest iron wire, a very large mass of iron or a rod of 1 centimetre diameter has no disturbing effect whatever. Its paralyzing effects are so remarkable, that if we place a flat piece of iron or other metal across the maximum line of force, where loud sounds are given out, the instant that this flat piece is moved, so that one or both edges touch or end in the minimum line of force, it becomes instantly neutralized, giving out no sound whatever, notwithstanding that a large mass of metal lies in the maximum line of force. We will now demonstrate by experiment the exceeding sensitiveness of the induction-balance to the smallest piece of metal, if it is in the maximum line of force, and no part of it touching the minimum; and also that by allowing either or both ends of this or a very large piece of metal to cut or end at the minimum lines of force, complete paralysis and consequent silence are produced*.

VI. There is a marked difference of the rapidity of action between all metals, silver having an intense rapidity of action. The induced currents from hard steel or from iron strongly magnetized are much more rapid than those from pure soft iron; the tones are at once recognized, the iron giving out a dull, heavy smothered tone, whilst hard steel has tones exceedingly sharp. If we desire to balance iron, we can only balance it by a solid mass equal to the iron to be balanced. No amount of fine wires of iron can balance this mass, as the time of discharge of these wires is much quicker than that of a larger mass of iron. Hard steel, however, can be easily balanced not only by steel but by fine iron wires, and the degree of the fineness of these wires required to produce a balance gives a very fair estimate of the proportionate time of discharge. The rapidity of discharge has no direct relation with its electrical conductivity; for copper is much slower than zinc, and they are both superior to iron.

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* This was fully demonstrated to the audience by numerous experiments.

We find that the induction-balance is exceedingly sensitive to all molecular changes which take place in all metals subjected to any of the imponderable forces. Thus we have already by its aid studied the effects on metals of heat, magnetism, electricity, &c., and of mechanical changes such as strain, torsion, and pressure; and I propose in some future paper to describe the remarkable results already obtained, and to demonstrate by experiment today to the Physical Society the results of these forces upon the induction-balance.

XIII. *Note on the Examination of certain Alloys by the Aid of the Induction-balance. By* W. CHANDLER ROBERTS, *F.R.S., Chemist of the Mint.*

[Plate XIII.]

SOME weeks since, Prof. Hughes showed me that equal volumes of various metals give widely different indications with the induction-balance. It appeared probable that a careful examination of a definite series of alloys would prove to be of interest; and as Prof. Hughes at once gave me the most generous assistance, teaching me the manipulation and controlling the results, I am able to submit the following observations to the Society.

The relative values of different metals as indicated by the induction-balance were given by Prof. Hughes in a paper read before the Royal Society on the 15th of May last. They do not accord with the values usually accepted as representing the relative conductivity of the respective metals; and this being the case, it became important to ascertain what relation the indications given by alloys, when under the influence of the induced current, bear to their electric conductivities, which afforded Matthiessen a basis for dividing them into groups[*].

A series typical of each group was therefore taken; the constituent metals were melted together in the requisite proportions; and the thoroughly mixed alloys were carefully rolled to a uniform thickness, usually 1·3 millim. Disks 24 millims. in diameter were then cut with the same punch; and these disks were placed in succession on one side of the balance, so that their bases lay exactly on a line midway between the primary and secondary coils, this having been found to be the plane of maximum force. The respective values of the alloys were ascertained either by introducing the sonometer into circuit, or by superposing a graduated wedge-shaped scale of zinc over the opposing coil of the balance, as has already been explained by Prof. Hughes.

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[*] British Association Report, 1863, p. 37.

The alloys of Lead and Tin were selected as an example of Matthiessen's first group. The results are recorded in the following Table, and are graphically indicated in the curve No. 1, Plate VI. The readings are those of the zinc scale*.

The Gold-Silver alloys, representing the second group, presented no difficulty of manipulation; and the observations were made on disks 1·3 millim. thick and 24 millims. in diameter. The results are given in the Table and in curve No. II.

The alloys of Tin and Copper, taken as representative of the third group, are peculiar. Their tints and fractures are widely different; and the series is interesting as having various industrial applications. As many of them are too brittle to roll, a block of each alloy 18 millims. square by 7 millims. thick was formed with the file. The results are given in the Table (p. 91) and on the curve No. III.

If the curves for Lead-Tin and Gold-Silver are compared with those given by Matthiessen † for the same alloys, their similarity will at once be evident. On the other hand, the induction-balance curve of the Tin-Copper series, while bearing some general resemblance to Matthiessen's curve of conductivity, differs essentially from it in certain parts. Matthiessen's curve falls rapidly from 93 (the conductivity of pure copper) to 9 (that of the alloy containing 85 *volumes* per cent. of copper). It then passes horizontally in a line which is approximately straight to 13, the conductivity of Tin.

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* The use of a scale of greater accuracy than the one employed may slightly alter some of the figures, but it can hardly change the general nature of the curves.

† *Op. cit.* p. 46, and Watts's Dictionary of Chemistry, vol. iii. p. 943.

	No.	Percentage composition.	Approximate formula.	Readings on Induction-balance.
TIN-LEAD.	1.	100 (pure tin).		67·5
	2.	77·37	$Sn_6 Pb$	62·0
	3.	69·60	$Sn_4 Pb$	59·0
	4.	53·20	$Sn_2 Pb$	52·5
	5.	36·30	$Sn Pb$	51·0
	6.	22·30	$Sn Pb_2$	47·0
	7.	12·50	$Sn Pb_4$	45·0
	8.	8·60	$Sn Pb_6$	46·0
	9.	0 (pure lead).		43·0
SILVER-GOLD.	1.	100 (pure silver).		225
	2.	99·97		209
	3.	99·90		205
	4.	99·50		192
	5.	99·10		170
	6.	98·02		148
	7.	94·93		115
	8.	90·00		84
	9.	81·40	$Ag_8 Au$	60
	10.	68·70	$Ag_4 Au$	48
	11.	52·30	$Ag_2 Au$	44·5
	12.	35·50	$Ag Au$	42·8
	13.	21·50	$Ag Au_2$	44
	14.	12·00	$Ag Au_4$	49
	15.	8·30	$Ag Au_6$	60
	16.	4·1		90
	17.	0 (pure gold).		150
COPPER-TIN.	1.	100 (pure copper).		167
	2.	89·00	$Sn Cu_{15}$	65
	3.	84·33	$Sn Cu_{10}$	51
	4.	79·02	$Sn Cu_7$	45
	5.	72·91	$Sn Cu_5$	40
	6.	68·28	$Sn Cu_4$	37
	7.	65·00		49
	8.	61·79	$Sn Cu_3$	83
	9.	51·84	$Sn Cu_2$	73
	10.	34·99	$Sn Cu$	73
	11.	9·73	$Sn_5 Cu$	82
	12.	0 (pure tin).		85

Note.—The Alloys were not annealed; and the temperature was about 15° C.

Some light would appear to be thrown on the difference between the two curves by the work of M. Alfred Rich* on the density of alloys of copper and tin. He showed that copper and tin contract in alloying, the contraction being regular from pure tin up to the alloy containing 38 per cent. of tin, the density of which is higher than that of pure copper.

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* *Ann. de Chimie et de Phys.* tome xxx. 1873.

M. Rich's experiments were conducted on alloys both in the form of powder and ingots; the latter have alone been given in the curve marked with his name on the Plate; and the relation between the two curves, especially at the points *a*, *a′*, *b*, *b′*, is too evident to need comment.

It may ultimately prove that if the alloys were rolled or compressed the curve would be modified; and, on the other hand, further experiments on the conductivity of the alloys may reveal points of identity between the conductivity and induction-balance curves; the part where the former from being vertical becomes horizontal would be especially worth examination. It may be well to point out that the alloys $SnCu_3$ and $SnCu_4$, which occupy critical positions on the induction curve, have been shown by M. Rich to be singularly free from the disturbing influence of liquation.

The work would appear to be interesting as showing that the induction-balance may afford a simple means for detecting variations in the molecular structure of alloys and for indicating allotropy in metals with greater accuracy than has hitherto been possible.

Practical application.—The possibility of ascertaining the standard fineness of alloys by the aid of electricity long ago occupied the attention of physicists. In 1823 M. Becquerel* suggested that trustworthy indications might be afforded by the electromotive force developed when the alloy is placed in an exciting fluid, together with an alloy of known composition. The subject was partially investigated by Œrsted in 1828†; and as its practical importance was further indicated by Gay-Lussac in 1830‡, I made a series of experiments in order to ascertain how far the more delicate appliances in use at the present day could be made available. The results, however, were not entirely satisfactory.

Prof. Hughes's Induction-balance rendered it possible to resume the research on a new basis. It is only necessary to glance at such a curve as that of the Gold-Silver series No. II. to be satisfied of the probability that certain parts of it, at least, would indicate minute differences of standard. I would therefore direct special attention to the series of alloys which lie between pure silver and silver alloyed with 5 per cent. of gold. These are shown in a separate curve, where the scale of percentages is more extended. Such alloys as Nos. 2 to 6 are known to refiners as doré; and No. 2 contains less than 2 grains of gold to the pound troy, a quantity which could not be extracted with profit by the ordinary operation of "parting." Small as the amount of precious metal is, its presence is clearly indicated on the induction curve, as are also the larger amounts of gold contained in Nos. 3–5.

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* *Ann. de Chimie et de Phys.* t. xxiv. p. 343 (1823).

† *Ibid.* t. xxxix. 1828, p. 274.

‡ *Instruction sur l'Essai des Matières d'Argent par la Voie Humide.* Paris, 1830.

Experiments are in progress on other series which promise to afford trustworthy indications; but of course the establishment of a method of verifying the composition of alloys of the precious metals must in part depend on the degree to which the presence of traces of foreign metals influence the accuracy of the results.

My object in these notes is not to insist on any particular application, but to bear testimony from a metallurgical point of view to the delicacy and simplicity of the instrument which Prof. Hughes has placed at our disposal; and I would offer him my sincere thanks for the liberal aid he has so readily given me.

XIV. *On* Professors Ayrton *and* Perry's *new Theory of the Earth's Magnetism, with a Note on a new Theory of the Aurora. By* H. A. Rowland, *Professor of Physics in the Johns Hopkins University*[*].

Some years ago, while in Berlin, I proved by direct experiment that electric convection produced magnetic action; and I then suggested to Professor Helmholtz that a theory of the earth's magnetism might be based upon the experiment. But upon calculating the potential of the earth required to produce the effect, I found that it was entirely too great to exist without producing violent perturbations in the planetary movements, and other violent actions.

I have lately read Professor Ayrton and Perry's publication of the same theory; and as they seem to have arrived at a result for the potential much less than I did, I have thought it worth while to publish my reasons for the rejection of the theory.

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[*] Read June 29th.

The first objection to the theory that struck me was, that not only the relative motion but also the absolute motion through space of the earth around the sun might also produce action. And to this end I instituted an experiment as soon as I came home from Berlin.

I made a condenser of two parallel plates with a magnetic needle enclosed in a minute metal box between them; for I reasoned that, when the plates were charged and were moved forward by the motion of the earth around the sun, they would then act in opposite directions on the enclosed needle, and so cause a deflection when the electrification of the condenser was reversed. On trying the experiment in the most careful manner, there was not the slightest trace of action after all sources of error had been eliminated.

But the experiment did not satisfy me, as I saw there was some electricity on the metal case surrounding the needle. And so I attacked the problem analytically, and arrived at the curious result that if an electrified system moves forward without rotation through space, the magnetic force at any point is dependent on the electrical force at that same point—or, in other words, that all the equipotential surfaces have the same magnetic action. Hence, when we shield a needle from electrostatic action, we also shield it from magnetic action.

This theorem only applies to irrotational motion, and assumes that the elementary law for the magnetic action of electric convection is the same as the *most simple* elementary law for closed circuits. Hence we see that, provided the earth were uniformly electrified on the exterior of the atmosphere, there would be no magnetic action on the earth's surface due to mere motion of translation through space.

In calculating the magnetic action due to the rotation, I have taken the most favourable case, and so have assumed the earth to be a sphere of magnetic material of great permeability, μ. It does not seem probable that it would make much difference whether the inside sphere rotated or was stationary; or at least the magnetic action would be greatest in the latter case;

and hence by considering it stationary we should get the superior limit to the amount of magnetism.

Let a be the radius of the sphere moving with angular velocity w, and let σ be its surface-density in electrostatic measure, and n the ratio of the electromagnetic to the electrostatic unit of electricity. Then the current-function will be

$$\phi=\frac{\sigma}{n}wa^2\int\sin\theta\,d\theta=-\frac{\sigma}{n}wa^2\cos\theta.$$

Hence (Maxwell's 'Treatise,' § 672) the magnetic potential inside the sphere is

$$\Omega=-\frac{8\pi}{3}\frac{\sigma}{n}war\cos\theta,$$

and outside the sphere

$$\Omega'=\frac{4}{3}\pi\frac{\sigma}{n}wa^4\frac{\cos\theta}{r^2}.$$

The magnetic force in the interior of the sphere is thus

$$F=\frac{8}{3}\pi\frac{\sigma}{n}wa,$$

or the field is uniform. If the electric potential of the sphere on the electrostatic system is V, we may write

$$F=\frac{2}{3}\frac{w}{n}V,$$

which is independent of the dimensions of the sphere.

In this uniform field in the interior of the sphere, let a smaller sphere of radius a' be situated; the potential of its induced magnetization will be

$$\Omega_1=\frac{\mu-1}{\mu+2}a'^3F\frac{\cos\theta}{r^2}.$$

Hence the expression for the potential for the space between the two spheres will be

$$U=\frac{2}{3}\frac{w}{n}V\left\{-r\cos\theta+\frac{\mu-1}{\mu+2}a'^3\frac{\cos\theta}{r^2}\right\};$$

and outside the electrified sphere it will be

$$U'=\frac{w}{n}V\left(a^3+2a'^3\frac{\mu-1}{\mu+2}\right)\frac{\cos\theta}{r^2}.$$

Let us now take the most favourable case for the production of magnetism that we can conceive, making $a'=a$ and $\mu=\infty$;

we then have

$$U' = \frac{w}{n} Va^3 \frac{\cos\theta}{r^2},$$

which is the potential of an elementary magnet of magnetic moment

$$\frac{w}{n} Va^3.$$

But Gauss* has estimated the magnetic moment of the earth to be

$$3\cdot3092\, a^3$$

on the millimetre mg. second system. Hence we have

$$V = 3\cdot3092 \frac{n}{w}$$

for the potential in electrostatic units on the mm. mg. second system. In electromagnetic units it is thus

$$V_1 = 3\cdot3092 \frac{n^2}{w};$$

and hence in volts it is this quantity divided by 10^{11}.

As the earth makes one revolution in $23^h\ 56'\ 4''$, or in 86164 seconds, we have

$$w = \frac{2\pi}{86164};$$

and

$$n = 299{,}000{,}000{,}000\dagger \text{ millims. per second.}$$

Hence the earth must be electrified to a potential of about

$$41 \times 10^{15} \text{ volts}\ddagger$$

in order, under the most favourable circumstances, to account

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[*] Taylor's Scient. Mem. vol. ii. p. 225.

[†] From a preliminary calculation of a new determination made with the greatest care, and having a probable error of 1 in 1300.

[‡] That this is not too great may be estimated from my Berlin experiment, where a disk moving 5,000,000 times as fast as the earth with a potential of 10,000 volts, produced a magnetic force of $\frac{1}{50000}$ of the earth's magnetism,

$$5{,}000{,}000 \times 10{,}000 \times 50{,}000 = 2{,}500{,}000{,}000{,}000{,}000,$$

which is of the same order of magnitude as the quantity calculated namely 61×10^{15}. It can be seen that this reasoning is correct, because the formulæ show that two spheres of unequal size, rotating with equal angular velocity and charged to the same potential, produce the same magnetic force at similar points in the two systems.

for the earth's magnetism. This would be sufficient to produce a spark in atmospheric air of ordinary density of about

6,000,000 miles!

Professors Ayrton and Perry have only found the potential 10^8 volts, or 400,000,000 times less than I find it.

It was this large quantity which caused me to reject the theory; for I saw what an immense effect it would have in planetary perturbations; and I even imagined to myself the atmosphere flying away, and the lighter bodies on the earth carried away into space by the repulsion. And, doubtless, had not Professors Ayrton and Perry made some mistake in their calculation by which the force was diminished 16×10^{16} times, they would have feared like results.

For according to Thomson's formula, the force would be equal to a pressure outwards of

$$r = \frac{V^2}{8\pi a^2},$$

which amounts to no less than

1,800,000 grms.

per square centimetre! or 10,000 kil. per square inch! Such an electrostatic force as this would undoubtedly tear the earth to pieces, and distribute its fragments to the uttermost parts of the universe. If the moon were electrified to a like potential, the force of repulsion would be greater than the gravitation attraction to the earth, and it would fly off through space.

For these reasons I rejected the theory, and now believe that the magnetism of the earth still remains, as before, one of the great mysteries of the universe, toward the solution of which we have not yet made the most distant approach.

In connexion with the theory of the earth's magnetism, I had also framed a theory of the Aurora which may still hold. It is that the earth is electrified, and naturally that the electricity resides for the most part on the exterior of the atmosphere—and that the air-currents thus carry the electricity toward the poles, where the air descending leaves it—and that the condensation so produced is finally relieved by discharge.

The total effect would thus be to cause a difference of potential between the earth and the upper regions of the air both at the poles and the equator. At the poles the discharge of the aurora takes place in the dry atmosphere. At the equator the electrostatic attraction of the earth for the upper atmospheric layers causes the atmosphere to be in unstable equilibrium. At some spot of least resistance the upper atmosphere rushes toward the earth, moisture is condensed, and a conductor thus formed on which electricity can collect; and so the whole forms a conducting system by which the electric potential of the upper air and the earth become more nearly equal. This is the phenomenon known as the thunderstorm.

Hence, were the earth electrified, the electricity would be carried to the higher latitudes by convection, would there discharge to the earth as an aurora, and passing back to the equator would get to the upper regions as a lightning discharge, once more to go on its unending cycle. I leave the details of this theory to the future.

Baltimore, May 30, 1879.

Appendix.—Since writing the above, Professors Ayrton and Perry's paper has appeared in full; and I am thus able to point out their error more exactly. Their formula at the foot of page 406 is almost the same as mine; but on page 407, in the fourth equation, the exponent of n should be $+\frac{1}{2}$ instead of $-\frac{1}{2}$, which increases their result by about 600,000,000, and makes it practically the same as my own.

Rotterdam, July 13th.

XV. *Notes from the Physical Laboratory of University College, Bristol. By* Prof. SILVANUS P. THOMPSON, *B.A., D.Sc.*[*]

I. *On the Source of Sound in the Bell Telephone.*

THE question has been at various times and in sundry places discussed whether the sounds emitted by the Bell telephone, when used as a receiver of currents, are caused by molecular vibrations in the instrument, or are due to vibrations executed by the thin iron disk as a whole. The former theory appears to have been started by Professor Bell himself[*] in order to account for the transmission of speech by instruments having very thick iron diaphragms, and by the instruments having no diaphragms at all. This view has been also upheld by the Comte Du Moncel in several communications to the learned societies of France. The other view appears to have been first distinctly put forward by Mr. W. H. Preece, in introducing the telephone to the British Association at Plymouth in 1877[†]; and it has for its most vigorous supporters M. A. Niaudet[‡] and Colonel Navez, the latter of whom has replied more than once to points raised by M. Du Moncel. It is a view which appears also to be supported by the recent experiments of Professor Hughes.

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[*] Read April 26, 1879.

The evidence now to be adduced, though not absolutely conclusive on the point at issue, opens out several fresh points of interest. It consists, in brief, of the results obtained by applying to the field of the telephone the experimental method of studying the so-called lines of force, originally due to Gilbert, and developed by Faraday. The details of the method followed by the present writer are identical with those described in his communication of June 23, 1878, "On Magnetic Figures," &c., and which consists in fixing permanently onto glass plates the figures obtained by iron-filings.

The figures obtained by means of iron-filings were resorted to with the view of ascertaining whether the changes in the magnetic field of the telephone were sufficiently marked to account for the alleged motions executed by the iron diaphragm, or whether they were such as to give any support to the molecular hypothesis.

The first step was to investigate the field of a bar-magnet when one pole was placed near a thin iron diaphragm.

It was known at the outset that a thin plate of magnetic matter might be magnetized in an enormous variety of ways. The magnetism might be distributed on the two faces, or in the manner known as *lamellar*; or, instead, any two points in the disk might be taken as conjugate poles; or any number of poles might be introduced; or, as in the magnets of M. Duter, the magnetization might be radially distributed, the central portion having one polarity, the other polarity existing all round the circumference. De Haldat showed that a variety of irregular magnetizations might be produced by touching steel plates with the pole of a powerful magnet; and the present author also found analogous effects to be produced by passing currents through steel disks. It therefore became a matter of some interest to determine the character of the magnetization of the telephone-disk.

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[*] A. Graham Bell, "Researches in Electric Telephony," Journ. Soc. Telegr. Eng. 1878, p. 414, vol. vi.

[†] Rep. Brit. Assoc. 1877, Plymouth, p. 13, W. H. Preece, C.E., "On the Telephone."

[‡] *Téléphones et Phonographes*, p. 92.

The figures exhibited two unsuspected features:—first, that when the diaphragm is larger than the end-face of the magnet, and even when it does not touch it, the distribution of the magnetism induced in the diaphragm is partially lamellar in character, partially radial. The central portion is magnetized almost entirely normally to its plane; the exterior portion is radially magnetized—a narrow annular region lying between these, in which the character of the magnetization is mixed. It was further observed that this neutral zone is of greater diameter when a larger magnet is employed, and that it enlarges also as the distance separating the magnet and the diaphragm is increased. It is more strongly marked as a region of separation between the central lamellar portion and the outer radially magnetized portion in the diaphragms of thin material than in those of thick. The position of this neutral annular zone is marked in fig. 1 (p. 101), which is a sectional diagram compiled from the figures produced by filings, by a point of flexure in one of the "lines of force" proceeding from the pole towards the diaphragm. The second feature noticed was that some of the outermost lines of force ran round to the front of the disk, entering it very near its circumference.

The next effect to be studied was that produced by the magnetic inductive action of a current traversing a coil of wire about the pole. For the convenience of obtaining the filing-figures upon glass plates but one turn of wire was employed, passing through holes drilled in the glass, and situated as is the coil in the Bell telephone over the pole of the magnet, the position arbitrarily found the most efficient in the construction of that instrument. In almost all modern elementary treatises on electromagnetics it is shown that the field of a plane closed circuit is equivalent to that of a lamellar magnetic shell of equal strength, or one which has an equal number of lines of force passing through the area it occupies. The result of passing the current around the pole of the magnet will therefore be, so far as the field *in the plane of the coil* is concerned, to increase or diminish respectively the number of lines of force due to the magnet by the number of lines of force due to the closed circuit, according as the direction of its field coincides with, or opposes that of the magnet. But the action is not so simple on the regions of the field outside the

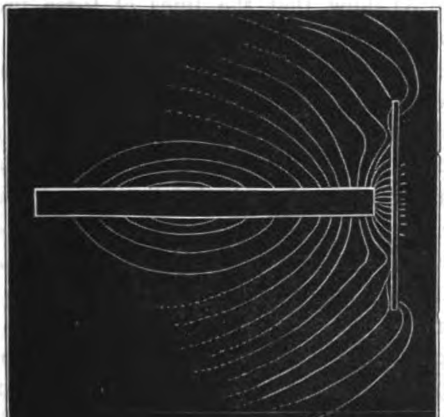

Fig. 1.

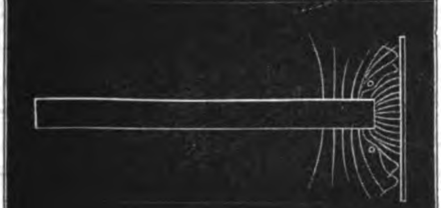

Fig. 2.

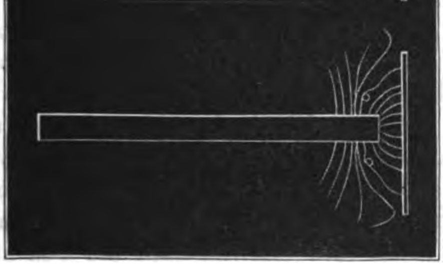

Fig. 3.

plane of the circuit. The *direction* as well as the *number* of lines of force may be altered, and this in a manner so complex as almost to defy calculation, especially if the mutual induction between this magnetic combination and the adjacent iron disk be taken into account. The figures obtained with filings when the current traversed the circuit in opposite directions (see figs. 2 and 3) show that the lines of force proceeding outwards from the pole were in fact thus altered both in number and in direction, and that, in addition to strengthening or weakening the field, the passage of the current had the effect of apparently thrusting the lines of filings forwards towards the iron disk or backwards from it. Moreover the region separating the two separate distributions of magnetism on the diaphragm was shifted on the passage of the current; being contracted in diameter when the current reinforced the magnetism of the pole, becoming enlarged when it passed in the opposite sense.

Knowing from the experiments of Joule and De la Rive that a portion of iron, when magnetized in a particular direction, grows longer in that direction and shorter in its transverse dimension, let us deduce what the effect will be on the diaphragm of a telephone of these two species of magnetization. If the magnetization were radial, the tendency would undoubtedly be, supposing the disk clamped circumferentially, to thrust the middle point of the disk backwards towards the magnet, and to give it a conical shape. If the magnetization, on the other hand, were lamellar, the tendency would be to make the diaphragm thicker, and to contract it over the area thus magnetized. In the actual case where the magnetization partakes of both characters, the two distributions being separated by a neutral zone, the tendency to each form would exist over the regions respectively affected. But the extent of these regions varies with the varying induction of the currents in the coil. Hence, while the total attraction varies, giving rise to oscillations of the diaphragm as a whole, the neutral annular line will also be continually shifting its position and predisposing the diaphragm to take up new nodal forms of vibration, thereby rendering the timbre corresponding to the complicated undulations of the currents arriving from the transmitter.

The result obtained may be regarded from another point of view. If a slight displacement of the iron disk, though unable to affect to any appreciable extent the strength of the magnetic field as a whole, alters its strength at any one point or in any narrow region, or if, even without altering the average number of lines of force in any part of the field, such a displacement shifts the position of some of the lines of force across a narrow region of the field, it may still exercise a considerable inductive action on a closed coil of wire lying in the region where the amount of shifting is greatest. For, since the induced electromotive force in a closed circuit is not proportional to the strength on an invariable magnetic field in which it lies, nor yet to all changes in its strength, but only to such changes as cause a greater or less number of lines of force to pass through the area within the closed circuit, it is evident that the inductive action will be strongest in coils of wire which lie in the region where there is most change in the direction of the lines of force. We have here the *rationale* of the empirical practice of the constructors of the telephone alluded to above—namely, that of using only a small coil of wire, and winding it upon a narrow bobbin placed upon the extremity of the magnet.

Conversely, the passage of a very feeble current through a coil so placed will produce a greater change in the effective intensity of the magnetic field between the core and the diaphragm than would be produced by the same current traversing a similar coil in any other region of the field; for here it has its greatest power to shift the position of the neutral zone, and to alter the distribution of magnetism in the diaphragm.

It would therefore appear unnecessary to form an hypothesis of molecular vibrations in the disk to account for the emission of sounds by the instrument. Such vibrations do in fact exist; but their existence does not necessarily prove that they play any important part in the production of the sound. And it must be remembered that, so far as the disk is concerned, they take place within the narrow range of the extreme positions possible to the neutral annular zone.

Two further experiments seem to confirm the conclusion derived from the foregoing observations. If a compound diaphragm be used, consisting of concentric annuli of thin iron fixed to a stretched membrane of paper, or if a small iron disk thus fixed be employed, as in Bell's earliest experiment and in some of the experimental telephones of M. Niaudet, a curious timbre is thereby imported into the voices of speakers, though their enunciation is very distinct. A similar result is found to follow the employment of small thick diaphragms. In each of these cases the disposition favours the lamellar distribution of the magnetism.

If, however, a compound diaphragm be employed, consisting of a number of radial pieces similarly fastened to a stretched membrane, tones are well rendered, but enunciation is not distinct. This result is also obtained when the diaphragm of iron is too large in proportion to its thickness. In these cases the greater part of the magnetism is radially distributed.

Whenever a complete theory of the telephone is framed, these are points which must be taken into account.

II. *On a new Variety of Magnetic Figures.*

De Haldat showed that it was possible to produce magnetic writing upon a steel plate by actually writing with the pointed pole of a powerful magnet, the writing being invisible until fine iron-filings were dusted over the plate. In the Physical Laboratory of University College, Bristol, a small circular saw has been found to afford a plate of suitable thickness and quality to produce good results. The latent characters remained for eight months after being inscribed.

While experimenting with these figures, it occurred to the author to try the effect of leading the current of a powerful battery into the plate and of writing on it with the other pole. This done, fine iron-filings were dusted over the plate; and on gently tapping it the writing became legible immediately. A small thin disk of steel which thus had a current passed through its centre exhibited afterwards a magnetism distributed in small concentric rings.

III. *On Magnetic Figures for Demonstration.*

For the production of magnetic figures filings of wrought iron are usually employed, though cast iron answers fairly. Finely powdered magnetic oxide is recommended by some writers, though it does not appear that its employment is attended with any great advantage. Professor A. M. Mayer took special pains* to produce filings of even quality from specially prepared Norwegian iron; but he says nothing about the size of filings he found best suited for the purpose. Faraday made the remark † that "large and also fine filings are equally useful in turn."

The author, desiring to obtain figures on a larger scale than usual, for purposes of class demonstration, used a number of small steel needles with success. In the case of thin elongated bodies such as these, the magnetic moment is great as compared with the mass; hence it was to be expected that filaments of fine soft iron wire would also yield a good result. Accordingly he had a quantity of fine iron wire gauze of 32 meshes to the inch cut to fragments. The filaments thus produced were scattered in the usual way by means of a pepper-box with perforated lid. The figures given by these filaments with large magnets possess very well-marked characters, and are decidedly superior to those made with ordinary filings.

IV. *On the Magnetic Behaviour of fixed Iron Filings.*

Having occasion to draw the attention of his students to the property of the lines of magnetic force as being at every point tangential to the position of a small freely-suspended magnet placed above them, the author, placing thus a small magnet over the filings fixed some time previously to glass, and from which the magnet producing them had been removed, noticed that they still retained their magnetic property. It then occurred to him to see whether they still possessed directive force as a whole, and found that they appeared capable of attracting and repelling a lightly suspended needle. A figure produced by a single bar-magnet and fixed permanently to a slip of card by gum was suspended lightly upon a needle-point by means of a glass cap. It set itself in the magnetic meridian, and was found capable of being deflected on the approach of a steel magnet. The fixed magnetic curves are therefore themselves magnets.

V. *Magnetic Figures of three dimensions.*

The writer has several times essayed to produce magnetic figures of three dimensions. The difficulty in producing them arises from the weight of the iron filings when unsupported, as they must be when the whole figure does not lie in one horizontal plane. With even the most powerful electromagnets the forms of the curves cannot be actually constructed in iron filings for more than a few millimetres length.

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* *Vide* American Journal of Science, 1872.

† 'Experimental Researches,' vol. iii. p. 398.

Attempts to float iron filings are also difficult, as there is no transparent liquid nearly approaching the density of iron. The writer has tried heavy paraffins and strong solutions of mercuric iodide in potassic iodide. Better effects were obtained, however, when iron filings were employed which had previously been coated with shellac varnish, and which therefore possessed greater buoyancy. The experiment is curious; but the difficulty of seeing across the forests of lines of filings reduces the observation to one of curiosity only.

Another process attempted consisted in plunging a small magnet into a soft paste of plaster of Paris and iron filings. The plaster shortly hardened; and then sections were cut in various directions. The figures observed, however, were poor; and no observations were made of any additional interest as the result of the attempt.

XVI. *The Distribution of Heat in the Visible Spectrum.*

By Sir JOHN CONROY, *Bart., M.A.*[*]

IN a paper "On the Distribution of Heat in the Spectrum," originally published in the Philosophical Magazine for August 1872, and since reprinted in a volume of 'Scientific Memoirs,' Dr. J. W. Draper states the theoretical reasons for supposing that all the rays in the spectrum have the same heating effect, in the following words:—"A given series of waves of red light impinging upon an extinguishing surface will produce a definite amount of heat; and a similar series of violet waves should produce the same amount; for though an undulation of the latter may have only half the length of one of the former, and therefore only half its *vis viva*, yet in consequence of the equal velocity of waves of every colour, the impacts or impulses of the violet series will be twice as frequent as those

[*] Read June 28.

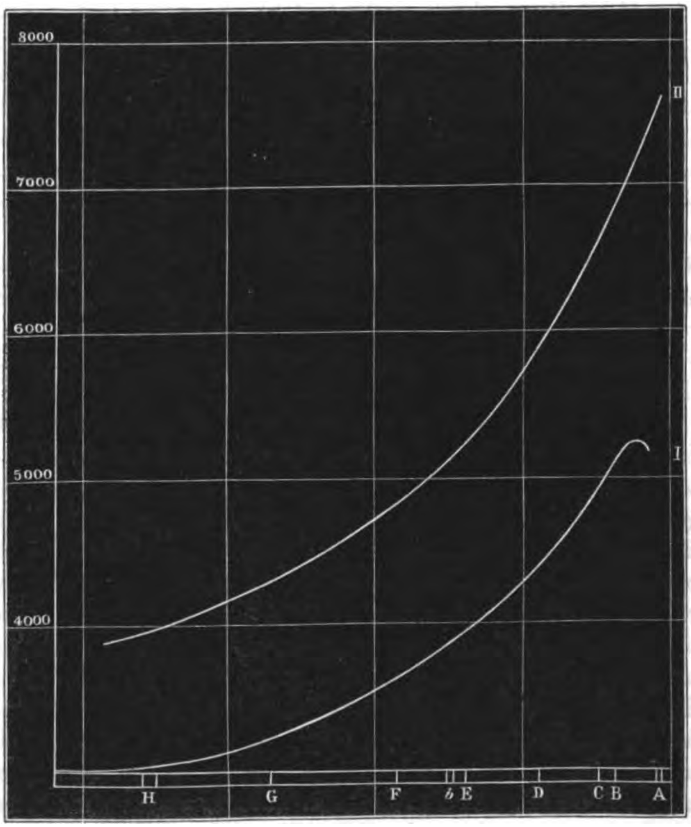
of the red. The same principle applies to any intermediate colour; and hence it follows that every colour ought to have the same heating effect."

Dr. Draper gives an account of some experiments he has made on the distribution of heat in the visible spectrum of sunlight. He finds that if the visible spectrum between A and H_2 be divided into two equal portions, and all the light of wave-lengths between 7604 and 5768 be collected together, and also all that of wave-lengths between 5768 and 3933, the heat-intensity of these two series of undulations as determined by the thermopile are equal.

The distribution of heat in the spectrum of sunlight had been previously experimentally investigated by Sir W. Herschel (Phil. Trans. 1800, p. 255), J. Müller (Pogg. *Ann.* cv. p. 337), Franz (Pogg. *Ann.* cxv. p. 266), Knoblauch (Pogg. *Ann.* cxx. p. 177, and cxxxvi. p. 66), Fizeau and Foucault (*Comptes Rendus*, xxv. p. 447, and reprinted in the *Annales de Chimie*, 5th series, xv. p. 363), Desains (*Comptes Rendus*, lxvii. p. 297, and lxx. p. 985), Lamansky (Pogg. *Ann.* cxlvi. p. 200). Similar measurements were made with the limelight by Desains (*loc. cit.*) and Lamansky (*loc. cit*), and of the electric light by Professor Tyndall (Phil. Trans. 1866, p. 1). They all found but slight indications of heat in the violet and blue regions of the spectrum, the amount increasing in the green, yellow, and red, and attaining a maximum at a point beyond the end of the visible spectrum.

The experiments were all made by placing a thermometer (one of the ordinary construction being used by Sir W. Herschel and MM. Fizeau and Foucault, and a thermopile and galvanometer by the other observers) in various parts of the dispersion-spectrum formed by prisms of either glass, rock-salt, or sylvine. As Dr. Draper points out in the paper already referred to, this method appears to be an essentially defective one, as, owing to the unequal dispersion by the prism of rays of different refrangibility, a greater number of undulations of different wave-lengths must have been incident upon the surface of the thermometer when it was placed in the red and yellow portions of the spectrum than when placed in the green, blue, or violet portions; and the amount of heat indicated by the instrument being in proportion to the amount of radiant energy incident upon its surface, the unequal dispersion of the prism would be sufficient to account for some difference in the heating effects produced by different portions of the spectrum.

A graphical method appearing to afford the readiest means of determining the probable effect produced by the unequal dispersion of the prism, a tracing was made, on paper divided into squares of $\frac{1}{10}$ inch, of the curve representing the intensity of the heat in different portions of the visible spectrum, as determined by MM. Fizeau and Foucault (*Ann. de Chim.* 5 sér. xv. p. 377)—the position of the fixed lines in the spectrum, as given by them, being marked on one edge of the paper, which was taken as the x axis, and a scale of wave-lengths in "tenth-metres" laid down at right angles to this, and the curve for the dispersion of the prism constructed in the ordinary manner.



At nineteen equidistant points in the spectrum the ordinates of the dispersion-curve were measured in wave-lengths; the difference between any two of them gave, approximately, the dispersion of the prism for that portion of the spectrum. A difference of wave-length of 100 was taken as the unit, and the difference between the values of the ordinates divided by 100 considered as a measure of the dispersion. The ordinates of the heat-curve of MM. Fizeau and Foucault at eighteen points in the spectrum, midway between those at which the ordinates of the dispersion-curve had been taken, were measured in tenths of an inch, these numbers divided by those representing the dispersive power of the prism, and the quotients taken as giving the true relative intensity of the heat at the different points of the spectrum.

Ordinates of the dispersion-curve.	Difference divided by 100 (α).	Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.
7260			
6790	4·70	22·4	4·7—B.
6320	4·70	19·0	4·0—C.
6000	3·20	15·9	5·0
5750	2·50	13·7	5·5—D.
5500	2·50	11·8	4·7
5260	2·40	10·3	4·3—E.
5070	1·90	8·8	4·6—*b*.
4900	1·70	7·5	4·4 —F.
4750	1·50	6·2	4·1
4610	1·40	5·0	3·5
4490	1·20	4·0	3·3
4380	1·10	3·3	3·0
4270	1·10	2·5	2·2—G.
4170	1·00	1·8	1·8
4070	1·00	1·3	1·3
4000	·70	1·0	1·4
3930	·70	·7	1·0—H.
3880	·50	·5	1·0

The Table shows that the numbers thus obtained for the intensity of the heat in different portions of the spectrum lie close together for the region between B and F, the maximum being near D, and that from F to H the intensity diminishes.

It would further appear that the curve for the distribution of heat of MM. Fizeau and Foucault is in reality a dispersion-curve, drawn to some scale of wave-lengths, for the particular prism used by them; and the diagram shows how very similar the curves for the intensity of the heat and for the dispersion of the prism are to one another.

The heat-curve reaches the x axis at a short distance on the more refrangible side of H_2, the wave-length of that line being 3932. The axis was taken as 3900 on the scale of wave-lengths to which the curve is drawn, and therefore the ordinate of the curve at B, measured in tenths of an inch, as proportional to the difference between this number and the wave-length of B, and the height of the ordinates at the other lines calculated on this assumption.

	Measured.	Calculated.	Difference.
B	2·10		
C	1·87	1·88	+·01
D	1·34	1·41	·07
E	·95	·97	·02
b	·88	·90	·02
F	·63	·68	·05
G	·22	·29	·07
		Mean difference ...	+·04

Considering the nature of the data, and especially the small scale of MM. Fizeau and Foucault's diagram (the portion representing the visible spectrum being only about 4 inches long), the measured and calculated numbers agree fairly well together.

The same process was applied to the curves given by Lamansky (Pogg. *Ann.* cxlvi. p. 200) for the distribution of heat in the solar spectrum, with flint-glass and rock-salt prisms.

Flint-glass prism.

Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.	
22·8	8·4	—D.
21·0	9·5	
19·0	11·8	—E.
17·5	19·4	
16·0	20·9	—*b*.
14·5	20·7	
12·7	15·8	
11·2	14·0	
10·3	14·7	—F.
9·8	14·0	
9·3	13·2	
8·5	12·1	
7·6	10·8	
6·8	8·5	
6·0	7·5	
5·0	7·1	—G.

Rock-salt prism.

Ordinates of the heat-curve (β).	$\frac{\beta}{\alpha}$.	
18·2	6·0	—D.
16·4	6·0	
14·3	7·9	—E.
13·0	9·2	
11·5	8·2	
10·3	7·3	—F.
9·3	7·1	
8·2	5·8	
7·0	5·0	
6·2	4·4	—G.

Assuming the curves to be the dispersion-curves for the prisms, the ordinates were measured and calculated as in the former case.

Flint-glass prism.

	Measured.	Calculated.
D	23·6	
E	17·8	16·0
b	15·4	14·6
F	10·2	10·8
G	4·0	

Rock-salt prism.

	Measured.	Calculated.
D	18·3	
E	13·7	13·4
F	9·8	10·3
G	6·0	

In none of the other measurements that have been made of the heat-spectra, as far as I am aware, are the positions of the solar lines stated; and therefore part of the data for eliminating the action of the unequal dispersion of the prisms is wanting.

An attempt was made to deal with Knoblauch's measurements of the solar spectrum, and with Tyndall's of that of the electric light, by the same method.

In both cases the experiments were made with rock-salt prisms; and these were assumed to have the same dispersive power as the one used by Lamansky, and the curve plotted accordingly. Two sets of measurements are given by Knoblauch; and the mean of these was taken for the following calculations.

Knoblauch's measurements.

Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.	
16·1	2·3	
12·0	3·6	—D.
8·7	3·9	
7·5	4·1	—E.
7·0	4·6	
6·8	4·8	
6·6	4·7	—F.
6·3	4·5	
6·0	4·6	
5·7	4·0	

Tyndall's measurements.

Ordinates of heat-curve (β).	$\frac{\beta}{\alpha}$.	
28·3	3·0	
21·0	3·0	
15·7	4·7	—D.
12·0	5·4	
8·2	5·7	—E.
6·5	4·3	
5·0	3·5	
3·5	2·5	—F.
2·0	1·7	
1·5	1·1	
·9	·6	

The nature of the available data is such that the only definite conclusions which it is possible to draw from these calculations are, that the distribution of heat in the normal spectrum differs greatly from that in the dispersion-spectrum, and that in the dispersion-spectrum the great calorific intensity of the red rays, and therefore in all probability of the invisible rays beyond them, is due to the action of the prism in concentrating these rays upon the face of the thermopile. The intensity of the heat in the different portions of the normal spectrum, except in the case of Lamansky's measurements with the flint-glass prism, apparently varies but little through a considerable space; and this affords some support to Dr. Draper's hypothesis, that every colour ought to have the same heating effect.

After I had finished these calculations, I found that G. Lundquist had investigated (Pogg. *Ann.* clv. p. 146), from Lamansky's measurements, the distribution of heat in the normal spectrum, and had shown that it differed greatly from the distribution in the dispersion-spectrum—the maximum intensity in the case of the flint-glass prism being near D, and in the rock-salt prism near E. He also found from Tyndall's measurements of the heat of the electric-light spectrum, that in the normal spectrum the maximum was near A.

Lundquist arrived at these results by a mathematical process, based on the same general principles as the graphical one I have employed.

XVII. *Measuring Polariscopes.*

By Professor W. Grylls Adams, *M.A.*, *F.R.S.*[*]

[Plate XIV.]

Some four years ago the description of a new measuring polariscope was communicated by the author to the Physical Society (see 'Proceedings,' vol. i. p. 152), in which the advantages gained are an extensive field of view combined with accurate means of measuring the rings and the separation of the optic axes in biaxal crystals. The peculiarity of the instrument consists in the arrangement of the two central lenses, one on each side of the crystal. These two lenses are plano-convex, very nearly hemispheres, and, with their flat surfaces inwards, form the two sides of a box to hold the crystal immersed in oil or a liquid; they are so placed that their convex surfaces form portions of the same spherical surface. The crystal is placed in the box at the centre of curvature of the spherical surfaces of the two lenses.

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[*] Read June 28.

Two instruments have since been made on this principle with certain important modifications. In one, made by Mr. Tisley for horizontal projection (Plate ~~XIV.~~ fig. 1), the polarizer is a Nicol's prism capable of giving a clear parallel beam of polarized light 2½ inches in diameter; the middle portion of the instrument (*i. e.* the box with the two equal central lenses for its two opposite sides) has an opening at the top, into which the crystal to be measured is inserted, and is adjusted to its right position by a cup-and-socket motion. When the angle between the optic axes is to be measured, the instrument is placed with its axis horizontal, the crystal is placed with the plane of the optic axis vertical, the box and crystal together are then turned about a horizontal axis at right angles to the direction of the axis of the instrument, *i. e.* at right angles to the plane of the optic axes: thus either of the optic axes of the crystal may be made to coincide with the centre of the field of view, where the spider-lines cross one another, the angle through which the box is turned being measured to minutes by means of a circle attached to it and a vernier attached to the fixed stand supporting the instrument.

XVI

A table-polariscope on the same principle has been made by Herr Schneider of Vienna, into which several important modifications have been introduced. A section and view of the instrument are shown in figs. 2 and 3.

The light falls on a plane mirror A, and is reflected into the instrument (which is placed with its axis vertical) through the first lens B, which is fixed on the tube in which the polarizer C is placed.

D, E, F, G, and H (fig. 2) are lenses through which the light passes; and K is a Nicol's prism (the analyzer). This part of the instrument forms a complete table-polariscope of considerable range. The Nicol's prism C and the lenses are supported each by means of two screws (shown in fig. 3), which may be moved upwards or downwards in two slots, and the lenses fixed in their proper positions. Between the plano-convex lenses E and F are the two central lenses L, M, two portions of a sphere, between which the crystal to be measured is placed. The crystal should be immersed in oil. The setting of the lens L has a screw on its surface which fits into a screw in the setting of the other lens M, the arm supporting the lenses being a flat piece of metal one sixth of an inch thick, which is placed between them before they are screwed together. This arm, I, is supported by a stout crosspiece T, in the form of the arc of a circle which has its centre at the centre of curvature of the two lenses: the arc subtends nearly a right angle at the centre. It passes through and is supported by two guides P, Q, and has on its outer surface a rack which works with a small toothed wheel turned by the milled head N.

On the upper face of the arm I is a train of toothed wheels, the setting of the lens M being provided with teeth, so that, by means of the milled head S on the outer wheel, the lenses and crystal may be turned about the axis of the instrument.

The guides P and Q are attached to the face of a vertical circle which may be fastened by screws to a circular graduated rim R; this rim is supported on and turns upon another fixed vertical circle, which is provided with verniers, so that the angle through which the rotating circle is turned can be measured to one minute. The fixed circle is fastened to and carried by the piece of tubing holding the two lenses E and F, which is supported between the two pieces of tubing carrying the upper and lower lenses.

In this instrument the crystal can be turned about each of three axes which are mutually at right angles to one another.

I. By turning the vertical circle and tubing to which it is attached, the crystal is turned about the vertical axis of the instrument.

II. By turning the train of wheelwork the crystal can also be turned about an axis passing centrically through the central lenses, and the plane of its optic axes may be brought into the desired position for measurement.

III. By turning the arc T by means of the milled head N, the axis of the central lenses may be made to coincide with the axis of the instrument; the plane of the optic axes is then parallel to the vertical fixed circle, the crystal being turned by this motion about an axis bisecting the obtuse angle between the optic axes.

IV. By turning the graduated circle with the arc T and the arm I attached to it on the face of the fixed vertical circle (*i. e.* round a horizontal axis perpendicular to the plane of the optic axes), and reading the verniers on both sides of the circle, the angles between the directions of the optic axes may be accurately measured.

XVIII. *A Mode of producing* Arago's *Rotation.*

By WALTER BAILY, *M.A.*[*]

[Plates XV. & XVI.]

ARAGO's method of producing rotation in a copper disk consists of suspending it by its centre so as to make it lie horizontally above the poles of a horseshoe magnet, and then rotating the magnet about a vertical axis. The rotation of the disk is due to that of the magnetic field in which it is suspended; and we should expect that if a similar motion of the field could be produced by any other means, the result would be a similar motion of the disk.

Possibly the rotation of the magnet may be the only practicable way of producing a uniform rotation of the field; but it will be shown in this paper that the disk can be made to rotate by an intermittent rotation of the field effected by means of electromagnets.

Suppose two magnetic poles to be below a plane sheet of a conducting substance capable of moving in its own plane. Each pole may be regarded as a small circular current parallel to the disk. The currents will be in the same or different directions according as the poles are of the same or different names. We will examine the effect of a change in the strength of either of the poles, in giving the sheet a tendency to move.

There are four cases; viz.—

1. Poles alike. One increasing.
2. „ „ One diminishing.
3. Poles unlike. One increasing.
4. „ „ One diminishing.

In case (1) the increasing pole induces in the portion of the sheet opposite to itself a circular current opposite in direction to the currents representing the poles. Hence this portion of the sheet is repelled by both poles. The repulsion from the increasing pole is perpendicular to the sheet, and gives it no tendency to move; but the repulsion from the other pole tends to move the sheet from the constant pole toward the one which is increasing.

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[*] Read June 28.

In case (2) the diminishing pole induces in the portion of the sheet opposite to it a circular current in the same direction as those representing the poles; so that this pórtion of the disk is attracted by both poles, and the disk tends to move from the diminishing pole toward the one which is constant.

In cases (3) and (4) it can easily be shown that the results are the opposite to those obtained in cases (1) and (2) respectively.

If one pole increases while the other diminishes, both tendencies to move are in the same direction, and the resulting tendency is the sum of the two.

The pole of an electromagnet made or broken is the extreme case of a pole increasing or diminishing.

Now conceive an even number of vertical bar electromagnets arranged in a circle with their upper poles in one horizontal plane, and a copper disk to be suspended above them; and the two magnets at the extremities of each diameter to be coupled together, and with a battery, so that each such pair of magnets forms a horseshoe electromagnet independent of all the others. Let P, Q, R, S be pairs of magnets at the ends of successive diameters. Make P, and then make Q so that the north pole of Q is adjacent to the north pole of P, and therefore the south pole of Q adjacent to the south pole of P. Then by case (1) the portion of the disk opposite the north pole of P is driven towards the north pole of Q; and a similar action takes place at the south poles. Now break P. By case (2) the portion opposite the north pole of P is again driven towards the north pole of Q, and so with the south poles. Continuing the action by making R and then breaking Q, making S and then breaking R, and so on, in each case making the adjacent poles similar, we get a series of impulses on the disk all tending to make it move in one direction round the axis of suspension. Hence the disk will rotate as in Arago's experiment.

In one extreme case, viz. when the number of electromagnets is infinite, we have the case of a uniform rotation of the magnetic field, such as we obtain by rotating permanent magnets.

In the other extreme case the number of pairs of electromagnets is reduced to two, and the number of batteries is also reduced to two. Let the poles of one pair of magnets be called a, a', and those of the other b, b'. Then the arrangement of the pole as seen from above is $\begin{smallmatrix} a & b \\ b' & a' \end{smallmatrix}$; and the successive states of these poles will be

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	ON	SN	SO	SS	OS	NS	NO	NN
SS	SO	SN	ON	NN	NO	NS	OS	SS

N, S, representing north and south poles respectively, and O representing that the pole is not magnetic. It appears that the change from 1 to 3 through 2 is nothing more than reversing $a a'$; and the change from 3 to 5 consists in reversing $b b'$. Similarly we pass from 5 to 7 by reversing $a a'$, and again from 7 to 1 by reversing $b b'$. The whole process is thus shown to consist in reversing $a a'$ and $b b'$ alternately.

In passing from 8 to 1 we see by case (1) that the parts of the disk over b, b' are respectively repelled from a, a'; and by case (3) we see that the same parts are respectively attracted to a', a. Again in passing from 1 to 2 we see by case (2) that the parts above a, a' are respectively attracted by b, b'; and we also see by case (4) that the same parts are respectively repelled by $b' b$.

The effect of each of these two changes is to make the disk tend to rotate in the direction $a b a' b'$. All the other changes may be shown to have the same effect; so that the disk will rotate in the direction $a b a' b'$.

If starting with the state (1) we reverse $b b'$ first, we should have the series of states as follows:—

1.	2.	3.	4.	5.	6.	7.	8.	1.
NN	NO	NS	OS	SS	SO	SN	ON	NN
SS	OS	NS	NO	NN	ON	SN	SO	SS

from which it may be easily shown that the disk would rotate in the direction $b a b' a'$.

The experiment with the four electromagnets may be readily performed by means of a commutator which will reverse the currents several times in a second; and a considerable rotation can be given to the disk.

The commutator which I constructed for the experiment consisted of a wheel of wood with a brass rim. This rim was completely cut through in places equidistant from each other; and ten tongues of thin copper pressed against the rim*. These tongues were in two groups of five; and in each group the distances between the contacts of the tongues with the rim were half the distances between the cuts in the rim. The distance between the two groups must be greater than the distance between the contacts.

Let A be a wire from the positive pole of one battery, and A' A'' be wires from the negative pole of the same battery; B a wire from the positive pole of the other battery, and B' B'' wires from the negative pole; and let $a\,a'$ be the ends of the coil round one pair of electromagnets, and $b\,b'$ the ends of the coil round the other pair. In the figures 1 to 4 the wheel is seen in four consecutive positions, with the tongues in contact with it; and the letters show with what wire each tongue is connected.

The contact of two tongues with the same section of the ring puts the wires to them into electric connexion.

The connexions are, in

Fig. 1. $A\,a$, $A'\,a'$, $B\,b$, $B'\,b'$,
Fig. 2. $A\,a'$, $A''\,a$, $B\,b$, $B'\,b'$,
Fig. 3. $A\,a'$, $A''\,a$, $B\,b'$, $B''\,b$
Fig. 4. $A\,a$, $A'\,a'$, $B\,b'$, $B''\,b$
Fig. 1. $A\,a$, $A'\,a'$, $B\,b$, $B'\,b'$.

Hence in passing from fig. 1 to fig. 2 the current through $a\,a'$ is reversed; in passing to fig. 3 that through $b\,b'$ is reversed. The current through $a\,a'$ is reversed again in passing to fig. 4, and that through $b\,b'$ is reversed again in passing to fig. 1. The commutator is thus seen to reverse each pair of magnets twice while rotating through the angle subtended by one division of the rim; so that with eight divisions one turn of the wheel reverses each pair of magnets sixteen times.

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* The number of cuts must not be less than seven. I used eight cuts.

If the wheel is rotated in the opposite direction, the series of magnetic states is obtained in the reverse order, and the disk rotates in the opposite direction. But there is a better method of reversing the motion of the disk—which is, to introduce an ordinary commutator into one of the circuits, either between the battery and the wheel, or between the wheel and the magnets. The reversal of this commutator reverses the motion, while the wheel is rotated continuously in one direction. Fig. 5 gives the arrangement of the connexions. A, B are the batteries; a, a', b, b' the electromagnets; C the ordinary commutator; and D the wheel.

It should be noted that the rotation of the disk is accompanied by the formation of induced currents whose intensity depends on the velocity of rotation, and whose effect is to diminish the rotation. These opposing currents are got rid of when the disk is suspended as a torsion-balance, and its deflection observed, as the currents will not be formed except when the disk is moving.

The effect on the disk might be much increased by placing four other electromagnets above the disk, each opposite one of the lower magnets, as connected with it, so that the lower pole of the upper magnet should be of the opposite name to the upper pole of the lower magnet. In fig. 6 one pair of magnets is shown with the opposite pair, and the wires connecting them. The disk is seen in section, balanced on a needle-point, between the two pairs of magnets. The other four magnets are not shown in the figure.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

FEB

NOVEMBER 1879.

XIX. *On the Conjugate Positions of two Circular Coils of Wire.* *By* W. GRANT, *Assistant in the Physical Laboratory, University College, London**.

[Plate XVII.]

WHILE recently engaged on some experiments on induction, I observed certain circumstances which I had not before noticed, and which seemed deserving of further attention. I was therefore led to inquire a little more closely into these matters; and although the investigation is by no means full or complete, I have obtained one or two results which I thought I might venture to lay before the Physical Society. The apparatus used in these experiments consisted, as at first arranged, of two coils of copper wire, one of which was connected in circuit with a battery of three Leclanché cells, and with a microphone which was actuated by a watch, while the other was connected with a telephone, in order that the induced currents, while passing through it, might render audible the beating of the watch which was used as the source of sound.

A modification of this arrangement was afterwards tried, a Grove's battery of twelve cells being substituted for the Leclanché battery, and a key being used for making and breaking the circuit. This was done in order to obtain greater inductive effects between the coils than could be obtained from the variations in the strength of the current which were caused by the action of the microphone. It was found, however, that with a little care in the adjustment of the coils, one cell gave sensibly as great an effect in the telephone as twelve cells; in subsequent experiments, therefore, the Grove's battery was discarded, and that of Leclanché again resorted to.

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* Read June 28th, 1879.

Now if two similar coils, connected as above described, are arranged with their planes parallel and their axes coincident, it is found that they may be separated to a considerable distance before the sounds which are heard in the telephone on making and breaking the circuit are obliterated. But it is also found that if the planes of the coils are kept parallel, the one in connexion with the telephone (that is, the secondary coil) may be placed in certain positions in the neighbourhood of the primary coil, and even in contact with it, without sounds being heard in the telephone. This happens when the mutual inductive effect between the two coils becomes zero; and when they are so placed as to fulfil this condition, they are said to occupy conjugate positions relatively to each other.

With the first arrangement of apparatus it was possible to place the coils so as to get complete silence in the telephone. With the powerful current from twelve Grove's cells and the key for making and breaking the circuit, however, the silence is not absolute; but in the positions which give a minimum of sound the sound is very faint, being just audible and no more. This faint sound may perhaps be accounted for partly because the different convolutions of wire in the secondary coil experience slightly different inductive effects from the primary one, and partly because it is difficult to adjust the positions of the coils with any great degree of accuracy without having special arrangements for the purpose.

Now it was found that the various conjugate positions in which the secondary coil could be placed in the neighbourhood of the primary one were situated in a path along which it could be moved either towards or away from the primary coil without sounds being heard in the telephone, but that with a slight deviation from this path to either side the sounds were again heard.

In order to ascertain whether the direction of the currents in the secondary circuit was reversed when the coil was moved from one side of the path to the other, a delicate reflecting galvanometer was substituted for the telephone, and the position of the coil so adjusted that on making and breaking the circuit no deflection of the galvanometer was observed. The coil was now moved slightly away from this position, say, towards the right; and the direction of the deflection of the galvanometer on making contact was noted, that on breaking being, of course, in the opposite direction. The coil was now moved towards the left to the other side of the path, and the direction of the deflections again observed; and it was found that they were now reversed. We may therefore infer that this path (which, if it could be fully traced, would of course constitute a surface of revolution about the axis of the primary coil) divides space into two regions, in one of which the inductive action of the primary coil has the opposite direction to what it has in the other.

This path appeared to be slightly curved; and it seemed as if a part of it might very readily be traced. The part which appeared to be best suited for this purpose was that along which the secondary coil has to pass while being moved away from contact with the primary one parallel to it to a position at some distance from it, as here the inductive effect is greatest, and therefore any deviation of the coil from the proper position in the path is most easily detected. As the coils are further separated, however, the position of the path becomes more difficult to trace, until at last we lose it altogether.

In order, then, to trace a curve which would represent this path, it was necessary to find several points in it whose positions could afterwards be accurately laid down. This was done by fixing the secondary coil in several positions successively and determining the position of a certain point in it with relation to certain fixed objects, by measurements which were afterwards used as abscissæ and ordinates in tracing the curve. These measurements were taken in inches; and their values are given in the annexed Table, where the columns headed x and y are those of abscissæ and ordinates respectively.

x.	y.	x.	y.
0·625	4·0	4·5	7·5
1·0	4·19	5·0	8·12
1·5	4·56	5·5	8·75
2·0	5·0	6·0	9·37
2·5	5·5	6·5	10·0
3·0	5·94	7·0	10·62
3·5	6·44	7·5	11·25
4·0	7·0	8·0	11·87

No special arrangement was used to adjust the parallelism of the coils, and only one measurement was taken for each number; hence the irregularity in the increase of the ordinates. The point whose position was determined in each case was the centre of the plane of the secondary coil; and that is the point which is situated in the curve when silence is maintained in the telephone.

The curve (Pl. XVII.) is that found in this way; and it represents the path which the selected point of the coil has to follow in order that silence may be maintained in the telephone. C_1 and C_2 are sections of the primary and secondary coils respectively; C'_2 and C''_2 represent the secondary coil in two other conjugate positions. The lines A, A and P, P represent the axis and plane of the primary coil. The points a, b, c, &c. are the intersections of the abscissæ and ordinates, and represent the successive positions occupied by the selected point of the secondary coil when the measurements were taken by means of which the curve was traced. As the coils became further separated, however, the position of the curve became less distinct; and so no attempt was made to trace it further.

If, now, we suppose the curve to rotate round the axis of the primary coil, a surface will be generated of which it is a section; and if we observe the conditions necessary for placing the secondary coil in the curve in the proper position for silence, we may place it in any part of the surface with a like result.

The reason why we are enabled to trace a curve in this way will be found by referring to the lines of force due to a circular current. These lines are represented by closed curves surrounding the section of the wire through which the current flows ; and they are given in Prof. Clerk Maxwell's work 'On Electricity and Magnetism,' vol. ii. pl. 18. If we draw tangents to them parallel to the plane of the circular current, it will be found that the points where they touch are situated in a curve somewhat similar to that which we have found by experiment. The two curves, however, will not be found to coincide exactly, except in the case where the secondary coil does not enclose a space—that is to say, when its diameter is infinitely small. With respect to the curve drawn through the points of contact of the tangents to the lines of force, it will be seen that the direction of all these lines between the curve and the axis of the circular current is away from, and that their direction on the other side of the curve is towards the plane of the circular current: hence on opposite sides of the curve their tendency is to produce currents in opposite directions.

If the curve is now supposed to revolve round the axis of the circular current, all lines of force enclosed by the surface generated will tend to produce currents in one direction, while all lines outside the surface will tend to produce currents in the opposite direction. Therefore, when the secondary coil is so situated with respect to this surface that as many lines of force pass through it in one direction as in the other, the resultant inductive effect on it will be zero; and this will be the case when it occupies any of the conjugate positions*.

It is evident from this, therefore, that we may combine the coils in several ways for the suppression of inductive effects :— first, by placing them close together face to face with their axes coincident, and so arranged that one of them may be moved across the face of the other parallel to their planes till a balance is obtained ; secondly, by placing them at some distance apart with their planes parallel and their axes coincident, and so arranged that if their planes are vertical each of them may be made to rotate round its vertical diameter; then if they are joined together when their axes are coincident, and combined like parallel rulers, they may be made to rotate together until a balance is obtained. With regard to this combination, it may be observed that the greatest inductive effect occurs when the planes of the coils are at the greatest distance from one another—and that as the planes approach, this effect gradually diminishes, until, when they are still at some distance, it becomes nothing.

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* In what precedes, the planes of the coils have been always supposed to be parallel to each other ; but it evidently follows from the reasoning here indicated that, if any set of parallel tangents be drawn to the lines of force and a curve be traced through the points of contact, an infinitely small coil would experience no inductive effect if it were placed with its centre anywhere in this curve, and with its plane parallel to the given set of tangents.

Another, and perhaps more convenient, way of combining them is to place them, as in the last case, with their planes parallel and their axes coincident, the distance between them being equal to, or a little greater than, the radius of either coil: then, if their planes are vertical, we may fix one of them in that position; and if the other is capable of rotating round its vertical diameter, it will be found that when it has rotated through 90° (that is, when the planes of the coils are at right angles) the inductive effects in the secondary circuit have ceased. If the coil is made to rotate through a few degrees to one side of this position, the currents induced in it will be in a certain direction; and if it is rotated to the other side, their direction will be found to be reversed.

As with either of these combinations we could pass from sound to silence, some experiments were made in order to compare the rate of diminution of the induced currents with the movements of the coils in passing from a maximum to a minimum of inductive effects.

For this purpose the coils were placed with their faces in contact and their axes coincident, the secondary one being joined in circuit with a reflecting galvanometer. In this position five observations were taken and the mean recorded. They were now separated until their planes were an inch apart, and a mean of five observations again taken; and this process was repeated at intervals of half an inch till the distance between them was increased to five inches.

They were now arranged as in the first combination, their faces being in contact during all the experiments; and while their axes were coincident, five observations were taken and the mean recorded. One of them was now slid over the other, the faces being still in contact, through a distance of half an inch and a mean recorded as before ; and this process was repeated at intervals of half an inch till a balance was established.

The second and third combinations were treated in the same manner, the coils being moved by steps of 10° at a time, and readings taken till a balance was obtained; and as the deflections were small in all cases, they were taken as being proportional to the strength of the currents.

The numbers given in the annexed Table are those found in the way indicated, the mean in each case being that of five experiments.

Axes of coils coincident.		First combination.		Second combination.		Third combination.	
Distance of planes.	Mean.	Distance of axes.	Mean.	Angle.	Mean.	Angle.	Mean.
0·625	88	0	88	0°	8	0°	17
1·0	77	0·5	84	10	7	10	17
1·5	52	1·0	70	20	7	20	17
2·0	38	1·5	57	30	6	30	16
2·5	28	2·0	40·6	40	5	40	16
3·0	22·5	2·5	30·8	50	4	50	15
3·5	17	3·0	20	60	3	60	12
4·0	13·4	3·5	10	65	0	70	9
4·5	10	4·0	0	...	...	80	5
5·0	8	...	...	...	...	85	2
						90	0

Note.—I may state here that I intend to continue this subject, and, when time permits, to trace some of the curves of equal induction.

XX. *On Magneto-Electric Induction. By* Frederick Guthrie *and* C. V. Boys, *Assoc. R. School of Mines.*—Part I.

[Plate XVIII.]

It is well known that the electric currents caused in a conductor by relative motion between that conductor and a magnetic pole are in such a direction as to impose a drag upon such motion. An ideal friction (that is, one without recoil) is called forth ; and, accordingly, in the experiments sometimes called Arago's such current-friction exhibits itself in the pursuit of the moving element by the one which is free to move, though originally at rest. Again, the same current-friction appears in the damping of the swing of the compass-needle by neighbouring metallic masses, and by the evolution of heat in the metallic disk revolving between magnetic poles.

It occurred to one of us that such pursuit as occurs in Arago's experiment might be made use of with advantage to measure the rate at which machinery is moving. For we have merely either (1) to connect a revolvable magnet with the machinery, and measure the angle through which it turns a copper plate in its near neighbourhood, which is restrained by increasing torsion, spring, or weight moment, or (2) to connect a copper plate with the machinery so as to revolve in the neighbouroood of a magnet restrained by similar means or by the earth's directive magnetism. The latter plan indeed might seem to have the obvious advantage that, as in the tangent-galvanometer, the amount of the effect is independent of the strength of the magnet. But this advantage is probably more than counterbalanced by the limitation of the angle of deflexion to less than a right angle, whether the magnet swing in a horizontal or a vertical plane.

We have in the following experiments adopted the plan of making a permanent steel or electromagnet revolve in the neighbourhood of the conductor, and governing the motion of the latter by the torsion either of a hair spring of a watch or fine platinum wire.

The accuracy of measurement which can thus be reached, and the greatness of the effects, led us to the hope that the electrical conductivity of liquids might be detected and measured by the same means—a result much to be desired, because, while the effect is due to the passage of currents in the liquid, such passage is wholly unaccompanied by electrolysis and its attendant incubus, polarization.

Before undertaking this latter investigation we have re-examined experimentally the results previously obtained with metallic conductors by others; and as our method differs somewhat from those previously employed, and our results are more extended and in some cases at variance with those of former experimenters, we venture to lay them before the Society *.

Our own Experiments.

Our experiments were mapped out as follows :—

Other things being the same,—

I. Vary rate of rotation.
II. Vary distance between the elements.
III. Vary diameter of disk.
IV. Cut metallic disk.
V. Vary thickness.
VI. Vary nature of metal of disk.
VII. Examine liquids in view of the determination of their conductivities.

I. *The Relation between Velocity and Deflexion.*—The apparatus used for establishing this relationship is, in its later form, shown in Pl. XVIII. fig. 1 one third the true size, the difference being that the torsion-thread was held by a clip attached directly to a glass shade instead of to the sliding arrangement shown.

The motive power was a Froment's electro-magnetic engine. The elastic band from the engine passed round one or other of the wheels on the vertical spindle, *p*, carrying a pair of magnets, *n s*, each $3\frac{3}{16}$ inches long × $1\frac{1}{8}$ × $\frac{1}{8}$, placed with their similar poles together. A screw on the spindle geared with a worm-wheel, *w*, with ninety-seven teeth. The time of a revolution of this could be measured either by watching a mark on the wheel pass a fixed pointer, or by its making a contact every revolution so as to sound a bell. The torsion-thread used was the hair spring of a watch, to which was attached the copper disk by means of a screw clip. The speed was altered by placing the driving-band on one or other of the pulleys on the vertical shaft, or on the Froment engine, or by altering the strength of the battery driving it, or by means of a friction-break.

The disk was graduated into degrees on its upper face, and the reading made by keeping a vertical edge, the eye, and the torsion-thread in one plane, and noting the division or part of one intersected by that plane, so as to avoid parallax.

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* At the recommendation of the Publication Committee of the Physical Society, we omit an historical summary of the previous investigations of others.

When the deflexion of the disk exceeds 20° or 30° the wire takes a "set," especially if it is kept long at its maximum twist. It appears completely to recover itself in time; but such recovery is at last so slow that it is preferable to redetermine the zero immediately after each experiment, and to allow for this, assuming that at the moment of observation (that is, when the disk is at its greatest excursion) the set in the wire is the same as it is immediately after when the zero is redetermined. When the deflexion is considerable it is impossible to keep it absolutely constant; accordingly the mean was taken of several maxima and minima, and the rate was registered after each set of observations. The means of the means of deflexion and rate were finally taken. When the rate is high and the deflexion considerable, the disk begins to swing, the axis describing a cone, until reading becomes impossible. This is possibly due to the vertical repulsion between the moving magnet and the disk not being quite symmetrical. In Experiment 1 all the data are given; in Table I. the means only are given.

Experiment 1.

O=0.

Maximum deflexion.	Minimum deflexion.
36°8	36°3
365	362
366	359
367	357
367	359

Index-wheel went five times round in 49 seconds.

Maximum deflexion.	Minimum deflexion.
364	359
363	360
364	361

Index-wheel went five times round in 49¼ seconds.

Maximum deflexion.	Minimum deflexion.
366	357
365	355

O=+2°.

The index-wheel going round once corresponds to ninety-seven revolutions of the magnet.

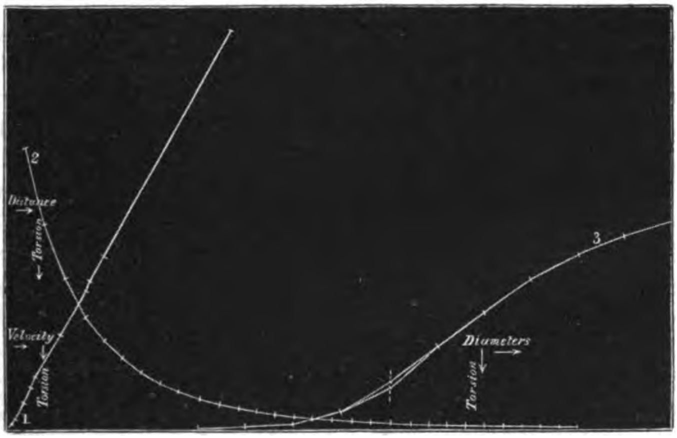
Allowing 2° for the effective set, we have a deflexion of 360°·25 caused by a velocity of 9·825 revolutions a second. In Table I. in the first five lines the rate-observations were simultaneous with the angle-observations; in the latter six they were taken as in the example given: each result is the mean of about eight observations.

Table I.

Angular-velocity, rotations per second.	Angular torsion.
0·258	9°·7
0·527	19·3
1·672	60·5
1·702	61·3
2·517	91·2
2·900	106·9
5·374	193·9
7·886	285·9
8·186	303·9
9·877	360·3
23·307	840·0

It appears from these numbers and from their graphic representation in line 1, fig. A, that the torsion varies directly as the rate. So exactly is this the case that we may in future with perfect confidence reduce from one rate to another or to

Fig. A.



any arbitrary common rate when the rate varies between experiments, to test the effect of variation of other kinds. And we have, if the magnets and torsion keep constant, a very exact and not inconvenient instrument for measuring the rate of rotation of machinery.

II. *The Relation between Distance and Deflexion.*—Two series of experiments were made to determine this relationship. We need only reproduce here the longest and most complete. The apparatus used was that shown in fig. 1, Plate XVIII., which is drawn to a scale one third true size.

The copper disk, *c*, was replaced by one of ebonite, to the lower surface of which the metal plate could be attached by a drop of weak gum. The copper disk used was only 0·27 millim. thick; so that all its parts might be considered at the same distance from the magnet. The magnet used was $3\frac{5}{16} \times \frac{1}{2} \times \frac{3}{16}$ inch. The upper end of the torsion-thread was attached by a screw-clip to the brass rod, and graduated in millimetres. This could be slid up and down, so as to vary the distance between the magnet and the disk. To get the absolute distance, the mica screen between them was removed, and a piece of copper exactly 1 millim. thick laid on the magnet, and the disk lowered till it just began to rest on the copper. Then the copper was replaced by the mica, and the first experiment made. Then the disk was raised 1 millim. by means of the graduated rod *r* for each succeeding experiment.

The ebonite disk and central clamp had been found to be unaffected by the fastest speed used. In order to avoid extravagant torsion, the rate of rotation was made less when the distance was small.

In Table II., column D shows the distance in millimetres between the upper surface of the magnet and lower surface of the copper disk; A shows the observed angles of depression; T shows the time in seconds for one revolution of the worm-wheel; A′ is the product of A and T. Since T is inversely and A is directly as the rate, A′ shows the pure effect of distance; it would be a constant if the distance did not vary. Each of the experimental numbers given, whether of angle or time, is the mean of never less than three observations. These observations of angle and time were simultaneous.

TABLE II.

D.	A.	T.	A′=A×T.
1	106°·8	13·7	1463·2
2	78·0	13·7	1068·8
3	75·6	10·2	771·1
4	57·0	10·1	575·7
5	68·3	6·5	443·9
6	50·9	7·2	366·5
7	55·1	5·3	292·0
8	43·3	5·3	229·5
9	36·3	5·3	192·4
10	45·1 44·6	3·6 3·6	162·4 160·6
11	36·1	3·7	133·6
12	30·5	3·7	112·8
13	25·6	3·5	96·6
14	21·0	3·6	75·6
15	17·9	3·4	60·9
16	15·5	3·5	54·25
17	13·3	3·6	47·88
18	11·6	3·5	40·60
19	10·4	3·5	36·40
20	9·0 10·0	3·5 3·6	31·50 36·00
21	8·0	3·5	28·00
22	7·1	3·5	24·85
23	6·2	3·5	21·70
24	5·2	3·5	18·20
25	4·8	3·4	16·32
26	4·6	3·4	15·64
27	3·5	3·6	12·60
28	3·5	3·6	12·60
29	3·0	3·6	10·80
30	2·6	3·6	9·36

The second numbers opposite distances 10 and 20 millims. were obtained after the completion of the set of experiments, by resetting the plate to the proper distances, to see if there had been any great change in the power of the magnets or the torsion of the thread.

On plotting out the curve of distance we find line 2, fig. A (p. 131).

III. *Variation in the Diameter of the Disk.*—For the study of this effect the same apparatus was used again, the only difference being that the magnet was only 2 inches long; and a series of disks of copper were cut of the same thickness as that used for finding the law of distance. They were held in the same way—by a drop of weak gum on the under surface of the ebonite disk. The distance was kept constant, namely 3 millims. In Table III., column Diam. gives the diameter of the disk in inches, column A the corrected mean deflexion of about four observations, column T the time of one revolution of the worm-wheel, got by taking the time of ten or twenty turns, during which the angles are read ; and column A′ is equal to A × T, and represents the torsion for constant velocity.

Table III.

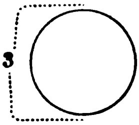
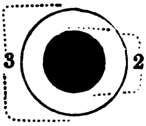
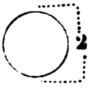
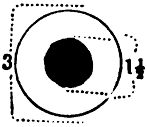
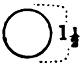
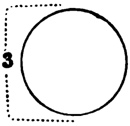
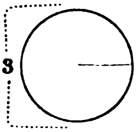
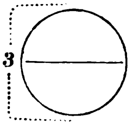
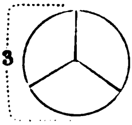
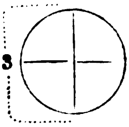
Diam.	A.	T.	A′=A×T.
1·00	0°·3	2·53	·759
1·25	0·9	2·79	2·511
1·50	2·6	2·82	7·332
1·75	6·5	2·83	18·395
2·00	10·7 12·5	3·99 3·90	42·69 48·75
2·25	21·5 21·4	4·02 4·09	86·43 87·53
2·50	30·8	4·03	124·1
2·75	38·7	4·06	157·1
3·00	46·1	4·01	184·9
2·25	50·9	4·00	203·6
3·50	50·0	3·90	218·4

The curve 3 in fig. A is the graphic representation of the above relationship. As was to be expected, there is a point of contrary flexure at about the region where the disk has a diameter equal to the length of the magnet.

IV. *Effects of Cuts in Disks.*—The same apparatus was used, and copper disks 3 inches in diameter, of the same thickness and at the same distance from the 2-inch magnet. In Table IV., A is the observed angle, T the observed time of rotation, and A′ is the product of A and T, and is proportional to the force for a constant rate.

The attached figures represent the way in which the disks were cut.

Table IV.

		A.	T.	A′=A×T.	
1.		44°·0	4·03	177·3	
2.		2·8	4·07	11·4	53·4
3.		9·6	4·37	41·95	
4.		16·0	4·31	68·96	77·0
5.		1·8	4·44	8·0	
6.		39·9	4·42	176·4	
7.		37·6	4·35	163·6	
8.		33·0	4·32	142·6	
9.		30·6	4·24	129·7	
10.		26·7	4·24	113·2	

In this Table the dark spaces in 2 and 4 mean absence of metal. The sum of the torsional effects of 2 and 3 or of 4 and 5 is equivalent to that of the disk in 1 having a concentric circular cut 2 inches or 1½ inch in diameter respectively—from which it will be seen that a concentric circular cut interferes with the induced currents far more than radial cuts of equal or even greater length. Experiment 6 is a repetition of experiment 1. The effect of the radial cuts in 7, 8, 9, and 10, though agreeing in kind with the experiments of Babbage and Herschel, is far less in amount; and this is noteworthy, as in their experiments the disks of lead were not cut completely to the centre, but a small space was left to unite the different parts together; while in our experiments the copper disk was cut completely through, the different parts being held in position by the ebonite plate above.

Taking the torsional effect of an uncut disk as 100, the following numbers show the effect of disks cut as in 7, 8, and 10, as obtained by

	7.	8.	10.
Babbage and Herschel . . .	83·2	72·5	44·8
Us	92·8	80·7	64·2

V. *Effect of Thickness.*—The examination of the effect of thickness is experimentally involved in that of distance, because, in increasing the thickness of a plate, it is impossible with our data to avoid change of distance of plane of effective mass. We have thought it best to employ for this purpose very thin disks cut out of a uniform sheet, and stuck with thin gum under the ebonite disk, at first one, then two, and so on up to six. Tinfoil was used 0·13 millim. thick, and 3·625 in. in diameter.

TABLE V.

No. of foils.	A.	T.	A′=A×T.	Δ A′.
1. ……	4·9	22·7	111·2	111·2
2. ……	10·5	21·3	223·6	112·4
3. ……	15·9	21·3	338·7	115·1
4. ……	22·0	21·3	468·6	129·9
5. ……	26·6	22·3	593·2	124·6
6. ……	30·0	24·6	738·0	144·8
1*a*. ……	6·3	23·2	146·2	

The column ΔA′ shows the increase in effect due to each foil; it also shows that the lower foils, being nearer the magnet, are more effective than the upper ones. A single foil 1*a* was afterwards hung at the distance of foil no. 6, and gave the result 146·2, agreeing with 144·8. The effect, then, of disks may be taken as directly proportional to their joint thickness, the mean distance remaining the same. There is probably a misreading with the four disks.

VI. *Effect of Nature of Metal.*—Sheets of copper, zinc, tin, brass, and lead having been rolled between the same rollers, were found to have the thickness as under :—

	millim.
Copper	0·89
Zinc	0·87
Tin	0·71
Brass	0·93
Lead	0·76

Disks 2·875 inches in diameter were cut out of the sheets, and fastened in the usual way by a drop of gum to the under surface of the ebonite disk. The results are given in Table VI.

TABLE VI.

Metal.	A.	T.	A′=A·T.	$\frac{A'}{t}$.	*r*.	$\frac{A' \times r}{t}$.
Copper...	78·5	21·8	1711·3	1923	·119	229
Brass ...	28·8	23·0	662·4	712	·423	301
Zinc ...	31·4	23·0	722·2	830	·326	271
Tin ...	13·0	23·3	302·9	427	·669	286
Lead ...	8·4	23·0	193·2	254	1·109	281

The column headed $\frac{A'}{t}$ is obtained by dividing the numbers in the previous column by the thickness of metal used. The numbers in the column *r*, give the resistance, in ohms of 100 inches, of wire drawn from the same metal that was rolled into sheets, and ·79 millim. in diam. These two columns multiplied together give the last column, which should be a constant if the deflexion is proportional to the conductivity. Now these numbers range from 229 to 301, which seems rather a wide divergence ; but in this case there are many considerations to be taken into account, in each of which errors may easily creep in, especially in the assumption that the specific conductivity of the same metal is the same whether it be rolled or drawn. The approximation to equality, obtained in the numbers in the last column, is all that could be expected.

It appears from the above experiments in I., II., III., IV., V., VI., that the torsion varies directly with the moment of the current.

VII. *Examination of Liquids.*—The electro-magnet seen in vertical section in Pl. XVIII. fig. 2, one third true size, was made to screw onto a vertical axis, so as to revolve in a horizontal plane. A 5-in. sulphuric-acid dish was held just above it by a separate stand to avoid any effects that vibration might cause; and into this dish the liquid to be tested could be put; a cover was also put over the dish. Mercury was first tried; and it soon began to rotate in the same direction as the magnet. Dilute sulphuric acid of the best-conductivity strength was next experimented on: a wood float that had been soaked in paraffin to make it float in the middle of the vessel served for an index to show motion of liquid. The float followed the magnet at the rate of about 15° an hour, but was always rather uncertain in its movement. The only noticeable feature of this experiment was, the fine sulphate-of-lead powder which had settled during the night arranged itself during the experiment in fine concentric rings, the circle immediately above the poles of the electro-magnet being bare. A solution of sulphate of copper showed no indications whatever.

As this method did not seem at all promising, the apparatus shown in vertical section in fig. 2 was next used. A glass bulb B was filled with the same sulphuric acid that was used in the last experiment, and hung by a silk thread in a small beaker *c* which acted as a screen; there was also a screen above. The electro-magnet *e* was made to revolve before the current was turned on, to see if there was any mechanical effect; but there was none at all; as soon, however, as the wires were connected with the battery the bulb B began to swing round, and stopped at about 180°. Here there was an unmistakable effect obtained with a non-metallic liquid conductor.

If, then, the torsion is proportional to the conductivity of the liquid in the vessel, which it undoubtedly is, we have the means of measuring the conductivity of liquids without using electrodes, and therefore without polarization. To see how the method worked, the preliminary apparatus shown in figs. 3 and 4 was constructed. N S is a double electro-magnet made from iron bar 1 in. wide and $\frac{1}{4}$ thick, bent as shown in horizontal section in fig. 3, so that the two poles are inside: with this arrangement the lines of force which intersect the cylindrical vessel B are almost straight and parallel. The vessel B was hung in the cylindrical screen *c* by a platinum wire 15 ft. long and 0·002 in. in diameter; and a small mirror was fixed to it for observing the reflexion of a small gas-flame. The liquid could be introduced by a pipette through a small hole in the side. Mixtures of sulphuric acid and water were made in the following proportions by weight, and were tested in the above apparatus:—

	a.	*b.*	*c.*	*d.*	*e.*
Acid	7	6	5	4	3
Water	3	4	5	6	7

They gave the following numbers, which represent the number of divisions on a scale of equal parts, at which the reflexion of the light could be seen when the mirror was in its position of mean deflexion.

a 2·1, *b* 2·6, *c* 5·5, *d* 6·8, *e* 8·9.

These numbers, being obtained only from a provisional apparatus, must not be considered to represent the accuracy of the process, but merely as showing that larger and more perfect apparatus is likely to give good results.

With regard to the validity of this method of determining the conducting-power of liquids a few words may be said. 1st. What will be the action of para- or dia-magnetic liquids apart from conductivity? Iron gives a greater turning effect than copper when placed over a revolving magnet, owing to its very great magnetizability and to its retaining magnetism a perceptible time; but whether liquids can acquire sufficient magnetism and retain it long enough to vitiate the results it is impossible to say; experiment will show. 2nd. Does the torsion of the wire really represent the twisting effect on the liquid *at rest*? It would seem at first that it does not; but a little consideration will show that no error in any way appreciable can be committed on the supposition that it does. The action is this: the rotating magnets cause the liquid in the vessel to rotate in the same direction; the friction between the liquid and the vessel tends to make the vessel rotate too; but it is restrained by the torsion of the wire. As the inertia of the liquid has to be overcome by a comparatively small force, its velocity will increase very slowly; and were there no friction against the sides of the vessel it would, if only left long enough, equal that of the magnet. But there is friction, which increases with the velocity; and therefore as the velocity increases the acceleration will diminish, and in time the velocity will be constant—that is, when the twisting force and the friction become equal—just as a light body when dropped in air soon attains a constant velocity. This velocity depends on the viscosity of the liquid; and it might be well said that two liquids having the same conductivity, but one viscous and the other mobile, would attain very different constant velocities, and, as the twisting force is directly proportional to the *relative* motion between the liquid and the magnet, that different results would be attained. This is quite true; but the question turns upon the velocity which the liquid attains. If it is itself comparable to the velocity of the magnet, then any changes in it will affect the result but if it is nothing compared with the magnet's velocity, then no error at all can be produced. Now on looking back it will be seen that the rotation of the best-conductivity acid in the 5-in. basin over a revolving electro-magnet turning about 3000 times a minute, was about 15° an hour; in other words, the magnet went about 4,320,000 times as fast as the liquid; and the total possible error on the supposition that the liquid is still, would be $\frac{1}{4320000}$ of the whole amount; therefore it may be safely neglected.

It appears, therefore, that the point to be borne in mind is that the torsion must not be measured till the liquid has attained its constant velocity; *i. e.* sufficient time must be allowed. This velocity and this time will be less the greater the friction or viscosity. But the introduction of glass threads, screens, or porous plates to increase the friction would do more harm by introducing uncertain electrical resistances than they could possibly do good: the only device which seems as if it might be of any use would be to make a jelly of the conducting liquid; but this is quite unnecessary.

We are engaged in measuring the conductivity of liquids by means similar to those above shown.

EXPLANATION OF FIG. A (page 131).

Curve 1. The abscissæ are in proportion to the velocities of rotation of a permanent steel magnet beneath a copper disk; the ordinates are in proportion to the angles at which the disk comes to rest. This curve appears to be a straight line, showing that the torsional moment is proportional to the velocity.

Curve 2. The abscissæ are in proportion to the distances between a thin copper disk and the upper surface of a permanent steel magnet revolving beneath; corrected for rate according to curve 1, the ordinates are in proportion to the angles at which the disk comes to rest.

Curve 3. The abscissæ are in proportion to the diameter of a thin copper disk, varying from 1 in. to 3·5 in. in diameter, turned by a revolving permanent magnet beneath, 2 in. long; the ordinates are in proportion to the angular deflexions. The double line shows the secondary observations about the region (the "pole") of the magnet where the effect of variation of diameter should be most marked. About this place there is an alteration in flexure.

XXI. *On the Determination of the Variation of the Thermal Conductivity of Metals with Temperature, by means of the permanent Curve of Temperature along a uniform thin Rod heated at one end. (Second Paper.)* *By* OLIVER J. LODGE, *D.Sc.*

THE first portion of a communication with the above title was printed in Part I. of the present volume of the Proceedings of the Society; but, a slip at the beginning having been noticed by Dr. Hopkinson which to some extent vitiated the results arrived at in the second part of the paper, this second half, consisting of sections 15–30, was suppressed, and not printed in the Proceedings of the Society, though it appeared in the Philosophical Magazine for April 1879. The present communication corrects and supplements the former paper, but, as it contains several references to the contents of the suppressed portion, this explanation is necessary.

The slip consisted in setting out with the ordinary equation to the curve of permanent temperature down a rod

$$\frac{d^2\theta}{dx^2}=\frac{\mathrm{H}p}{kq},$$

which is true when k is constant, and working with it as if it were equally valid when k is assumed to be variable. The oversight was inexcusable, because in § 2 of the paper referred to I indicated, for the sake of completeness, the ordinary way in which this fundamental equation is obtained, and I thoughtlessly wrote the gain of heat per second by an element of the rod at a distance x from the origin as

$$kqd\frac{d\theta}{dx}$$

as usual, instead of what it obviously becomes when k is not considered constant,

$$dkq\frac{d\theta}{dx}.$$

The term containing $\frac{dk}{d\theta}$ which I omitted is but a small one, however, and does not make very much difference to the result: hence the sections 16–20, though superseded by the present communication, are not exactly incorrect, but are first approximations; and the curve A spoken of in § 21, and drawn in Plate VII., does represent the character of the curve of temperature down a long iron rod *in vacuo*, with one end 300° hotter than the other. But no calculation of the variation coefficient of conductivity is likely to be possible by means of equations from which the term $\frac{dk}{d\theta}$ had been omitted.

30. With the exception of the correction now indicated in equations (1) and (3), the first fifteen sections of the paper are quite unaffected by the slip, and may remain as they stand, except that I have now a little more to say on the subject of §§ 5–9.

Professor Tait has been kind enough to send me a copy of his researches on "Thermal and Electric Conductivity," read before the Royal Society of Edinburgh in March and June 1878; and I find that he has given up his enticing speculation as to the inverse variation of thermometric conductivity with absolute temperature—and in fact that he believes iron to be possibly exceptional in the inverse connexion of conductivity and temperature, all other metals which he has subjected to experimental observation showing a slight *increase* of conductivity as the temperature rises. Prof. Tait's results are thus in opposition to the results of Prof. Angström for copper; but since Prof. Angström, in the interpretation of his very ingenious method of experiment, used the ordinary Fourier equations, formed on the supposition that k is constant, and that rate of cooling is proportional to excess of temperature, Prof. Tait does not consider his observations competent to decide a point as to the variability of k. Without venturing an opinion of my own on the subject, it is evident that this opposition is an additional reason for attacking the important question of the law of the variation of thermal conductivity with temperature.

Prof. Tait finds that a linear function of the temperature, $k=a+bt$, will express the value of the thermal conductivity according to his experimental results, at least in their present preliminary stage; and we saw in § 8 that Prof. Forbes's results for iron could be expressed nearly as

$$k=\cdot 207(1-\cdot 00144\,t);$$

hence instead of the law of variation of *thermometric* conductivity,

$$\frac{k}{c\rho}=\frac{A}{b+t},$$

assumed in the former paper, I shall now assume as the law of the variation of *thermal* conductivity,

$$k=b\kappa\left(1+\frac{1}{b}\,t\right)=\kappa(b+t),$$

where b is positive for metals whose conductivity increases with temperature, but both b and κ are negative for those whose conductivity diminishes with temperature, like iron.

It will be necessary to reckon temperature, as before, from the temperature of the enclosure $(v_0-274=t_0)$ as zero; so instead of $b+t$ we write $m+\theta$, where $m=t_0+b$.

Similarly, for the law of variation of specific heat and density, we may assume (see § 8),

$$c\rho=\beta\sigma\left(1+\frac{1}{\beta}\,t\right)=\sigma(\beta+t)=\sigma(n+\theta),$$

where $n=t_0+\beta$ and is always positive, because the specific heat of all metals except mercury appears to *increase* with temperature.

§§ 10–15 remain unaltered; but I shall only attempt the case of a rod *in vacuo*, as in air the integration appears impracticable. §§ 25–29 also remain practically unaltered.

31. We start, then, with the following equations to the variables, of which the numbering agrees with that of the corresponding equations in the preceding paper, whether they are the same or different; but the numbers are put into square brackets when for any reason the equation is modified. Numbers above 32 characterize those equations which did not appear in the former paper.

$$k=\kappa(m+\theta), \qquad [4]$$

$$\therefore \frac{dk}{d\theta}=\kappa=\frac{k_0}{b}=\frac{k_0}{m+t_0}, \qquad (33)$$

$$c\rho=\sigma(n+\theta), \qquad (34)$$

$$\mathrm{H}=\frac{qc\rho}{p}\dot{\theta}, \qquad (2)$$

$$\dot{\theta}=\mathrm{P}a^{v_0}(a^\theta-1)=\mathrm{R}(a^\theta-1). \qquad (5')$$

And the fundamental equation is

$$dkq\frac{d\theta}{dx}=\mathrm{H}pdx\,; \qquad [1]$$

or, neglecting the expansion by heat of the cross section q,

$$\frac{d}{dx}k\frac{d\theta}{dx}=\frac{\mathrm{H}p}{q}=c\rho\dot{\theta}. \qquad [3]$$

This may be written

$$\frac{d^2\theta}{dx^2}+\frac{1}{k}\cdot\frac{dk}{d\theta}\left(\frac{d\theta}{dx}\right)^2=\frac{c\rho}{k}\dot{\theta},$$

whence, by help of [4] &c.,

$$\frac{d^2\theta}{dx^2}+\frac{1}{m+\theta}\left(\frac{d\theta}{dx}\right)^2=\frac{\sigma}{\kappa}\cdot\frac{n+\theta}{m+\theta}\cdot\dot{\theta}. \qquad [11]$$

32. To integrate this once, make the substitution $\left(\frac{d\theta}{dx}\right)^2=z$, when [11] becomes

$$\frac{dz}{d\theta}+\frac{2z}{m+\theta}=2\frac{\sigma}{\kappa}\cdot\frac{n+\theta}{m+\theta}\cdot\dot{\theta}$$

which is of the form

$$\frac{dz}{d\theta}+\mathrm{P}z=\mathrm{Q},$$

whose integral is

$$z=e^{-\int \mathrm{P}d\theta}\left\{\int e^{\int \mathrm{P}d\theta}.\,\mathrm{Q}d\theta+\mathrm{C}\right\}.$$

Hence the first integral of [11] is

$$\left(\frac{d\theta}{dx}\right)^2=\frac{2\sigma}{\kappa(m+\theta)^2}\int_0^\theta(m+\theta)(n+\theta)\dot{\theta}\,d\theta,$$

the limits being taken to suit a rod whose length is unlimited (that is, one in which θ and $\frac{d\theta}{dx}$ vanish together)—a condition which is necessary for simplicity, as is explained in § 17.

We may now insert the value of $\dot{\theta}$ from (5) and write the above thus,

$$\frac{\kappa}{2\mathrm{R}\sigma}\left\{(m+\theta)\frac{d\theta}{dx}\right\}^2=\int_0^\theta(m+\theta)(n+\theta)(a^\theta-1)d\theta$$

$$=\frac{a^\theta-1}{\log a}\{\mathrm{M}+(mn-\mathrm{M})\theta+\theta^2\}-\mathrm{M}\theta, \quad . \quad [12]$$

where for shortness the letter M is written instead of the constant

$$mn-\frac{m+n}{\log a}+\frac{2}{(\log a)^2}.$$

33. So far we have proceeded with perfect accuracy; but in order to integrate the equation any further it is necessary to expand $a^\theta-1$ and neglect higher powers of $\theta\log a$, as was done and justified for all probable values of θ in § 12. The approximation used is

$$a^\theta-1\bumpeq\theta\log a\{1+\tfrac{1}{2}\theta\log a+\tfrac{1}{6}(\theta\log a)^2(1+\tfrac{1}{8}\Theta\log a)\}. \quad (7')$$

Introducing this into [12], after writing γ for the small correction factor $\frac{1}{8}\Theta\log a$, and α for the perpetually occurring constant $\log a=\log_e 1{\cdot}0077=\frac{1}{130}$, it becomes

$$\frac{\kappa}{2\mathrm{R}\sigma}\left\{(m+\theta)\frac{d\theta}{dx}\right\}^2=$$

$$=\theta\left(1+\tfrac{1}{2}\alpha\theta+\frac{1+\gamma}{6}\alpha^2\theta^2\right)\left(\mathrm{M}+(mn-\mathrm{M})\theta+\theta^2\right)-\mathrm{M}\theta$$

$$=\tfrac{1}{2}\alpha\theta^2(\mathrm{A}+\mathrm{B}\theta+\mathrm{C}\theta^2), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad [13]$$

where

$$\left.\begin{aligned} A&=\frac{1}{\alpha}\left(\alpha mn+m+n-\frac{2}{\alpha}\right), \\ B&=\frac{2-\gamma}{3}(m+n)+\frac{1+\gamma}{3}\left(\alpha mn+\frac{2}{\alpha}\right), \\ C&=1+\frac{1+\gamma}{3}\left(m+n-\frac{2}{\alpha}+\theta\right)\alpha. \end{aligned}\right\} \quad . \quad (35)$$

(Remember that $\frac{1}{\alpha}=130$, and that $\gamma=\frac{1}{8}\alpha\Theta=\frac{\Theta}{1000}$ practically.) The coefficient C, therefore, is not quite constant, but depends upon θ. The dependence, however, is very slight, since $m+n$ is usually a large number, and α a small fraction; and it will be quite sufficient to write the average value $\frac{1}{2}\Theta$ instead of θ in the brackets of C, and thus to make it a constant. The usual relative sizes of the three constants are, A numerically much larger than B, and B numerically much larger than C.

For metals whose conductivity increases with temperature all three constants are positive; but for metals like iron, whose conductivity decreases with temperature and for which therefore m is negative, A and B are certain to be negative, while C is very likely to be positive but small. We can avoid this change of sign by noticing that when m is negative κ is also negative; hence, if we bring κ over to the right-hand side of [13], we shall get new constants, $\frac{A}{\kappa}$ and $\frac{B}{\kappa}$, which will be always positive, and $\frac{C}{\kappa}$, which is positive for all metals which have κ positive, but which is generally negative for those which, like iron, have κ negative. In all the following equations, wherever A and B appear alone, they may be always reckoned positive, because the κ has merely been cancelled out. In order that C may be negative, it is necessary not only that m shall be negative, but also that it shall be numerically greater than $n+\frac{1}{2}\Theta+\frac{1}{\alpha}$; hence this is the condition which will allow $\frac{C}{\kappa}$ to be positive when κ is negative.

34. It may be useful to calculate the numerical value of these constants for such metals as we at present possess any experimental data for. We will assume the temperature of the enclosure to be 0° C. (so that $m=b$ and $n=\beta$, see § 30),

and the highest observed point of temperature Θ on the rod to be 100° C.: then the following Table contains the values of the constants A, B, and C for iron and copper, together with certain ratios which will be used later. The row of numbers deduced from the experiments of Forbes, confirmed by Tait, are probably nearly accurate; the others are subject to discount.

Metal.	Value of β or n according to Bède and Fizeau.	Value of b or m according to		A.	B.	C.
Iron ...	760	Forbes ...	− 700	− 578,000	− 1,360	+ ·577
		Ångström	− 640	− 504,600	− 1,200	+ ·746
Copper..	2200	Ångström	− 940	−1,964,000	− 5,063	+ 3·4
		Tait	+2000	+4,912,200	+15,166	+12·3

$\frac{2A}{B}=r.$	$\frac{2C}{B}=s.$	$rs=\frac{4AC}{B^2}.$
+850	−·00085	− ·7225
+841	−·00124	−1·0428
+775·7	−·00134	− 1·0394
+647·8	+·00162	+1·0494

35. The integral of equation [13] may be written down without difficulty; and it constitutes the equation to the permanent curve of temperature down a long thin uniform cylindrical metal rod with a blackened surface heated at one end *in vacuo*.

$$\left[\log\frac{\left\{\frac{2A}{\theta}+B+2\sqrt{A}\sqrt{\left(\frac{A}{\theta^2}+\frac{B}{\theta}+C\right)}\right\}^{m\sqrt{\frac{\kappa}{A}}}}{\left\{2C\vartheta+B+2\sqrt{C}\sqrt{(A+B\theta+C\theta^2)}\right\}^{\sqrt{\frac{\kappa}{C}}}}\right]_{\Theta}^{\theta}=\sqrt{(R\sigma\alpha)}x.[14]$$

For a much simpler and practically useful form of this equation see equation (40) § 38.

36. The expression on the left-hand side of this equation may be written in various ways; and its form is slightly different according as C is of the same or opposite sign to κ.

For the case when C and κ have the same sign (which according to Prof. Tait is probably most usual) we may write it conveniently

$$\left[m\sqrt{\left(\frac{\kappa}{A}\right)}\sinh^{-1}\frac{\frac{2A}{\theta}+B}{\sqrt{(4AC-B^2)}}-\sqrt{\left(\frac{\kappa}{C}\right)}\sinh^{-1}\frac{2C\theta+B}{\sqrt{(4AC-B^2)}}\right]_\Theta^\theta=\sqrt{(R\sigma\alpha)}x. \quad . \quad . \quad (36)$$

But for the case when the signs of C and κ are opposite (as for iron), it becomes

$$\left[m\sqrt{\left(\frac{\kappa}{A}\right)}\cosh^{-1}\frac{\frac{2A}{\theta}+B}{\sqrt{(B^2-4AC)}}-\sqrt{\left(\frac{-\kappa}{C}\right)}\cos^{-1}\frac{2C\theta+B}{\sqrt{(B^2-4AC)}}\right]_\Theta^\theta=\sqrt{(R\sigma\alpha)}x. \quad . \quad . \quad (37)$$

Whether we write $\cos^{-1}$ or $\sin^{-1}$ in this equation only affects the sign of the term containing it. In equation (36), if 4AC is less than B^2 (which is unlikely), the terms in the denominators must be transposed and $\cosh^{-1}$ written for $\sinh^{-1}$.

Of the two terms in the brackets of these equations the first is by far the most important, and in the former paper is the only one which appeared (see equation 14); the second term has only a small effect on the curve, and this effect vanishes with C[*]. The occurrence of the inverse circular function in the curve of temperature along metals whose conductivity diminishes with temperature is peculiar; but C must always be very small for such metals; so that it does not make much difference. And this is a good thing, because when m is negative the θ occurring in the expression called C is of more relative importance, and C is therefore not so constant as when m is positive: but the approximation is always pretty good, the worst possible case being that of a supposititious metal with $m=-\left(n-\frac{2}{\alpha}\right)$, for which $C=1+\frac{\theta}{355}$.

37. We have thus obtained the equation to the curve of temperature expressing x as a function of θ. What we have now to do is to show how, from experimentally observed corresponding values of θ and x, the constants A, B, C can be determined, or at least such of them as are required for the determination of the constants κ and m. In order to deter-

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[*] To avoid a possible misunderstanding, it may be well to say that this does not mean that the *term* vanishes, because of course it becomes infinite; but it means that the *effect* of the term vanishes, because when the limits are put in, the two things subtracted from one another are equal.

mine these constants we must apparently use some method of successive approximations; and the precise method adopted will probably be a matter of taste. I may, however, suggest the following as certainly applicable to the case of κ and m positive, *i. e.* to equation (36), and as inferentially applicable to the other case (37) if we can get some imaginary quantities to cancel each other. We will therefore proceed with the general case, and not trouble about whether the quantities are real or imaginary.

The quantity $\dfrac{\dfrac{2A}{\theta}+B}{\sqrt{(4AC-B^2)}}$ is always pretty large, even when θ has its maximum value Θ; call it y. Then writing $\sinh^{-1} y = \log(y+\sqrt{y^2+1})$, we see that, since y is large, y^2+1 is practically the same as y^2, and therefore that $\sinh^{-1} y \triangleq \log 2y$ to all intents and purposes. This approximation is always very close; and it is perfectly accurate when $rs=1$, *i. e.* when $4AC=B^2$. That this is not likely to be far from the case is illustrated in the Table (§ 34).

The quantity $\dfrac{2C\theta+B}{\sqrt{(4AC-B^2)}}$, however, is not large at all, but has a value not very different from unity. We had better therefore take its $\sinh^{-1}$ in the logarithmic form, and write (36) with the limits put in

$$m\sqrt{\frac{\kappa}{A}}\log\frac{\dfrac{2A}{\theta}+B}{\dfrac{2A}{\Theta}+B}$$

$$-\sqrt{\frac{\kappa}{C}}\log\frac{2C\theta+B+\sqrt{4AC+4BC\theta+4C^2\theta^2}}{2C\Theta+B+\sqrt{4AC+4BC\Theta+4C^2\Theta^2}}=\sqrt{(R\sigma\alpha)}x \qquad . \quad . \quad . \quad (38)$$

or, writing $\dfrac{2A}{B}=r$ and $\dfrac{2C}{B}=s$,

$$\left[\frac{1+\dfrac{r}{\theta}}{1+\dfrac{r}{\Theta}}\right]\cdot\left[\frac{1+s\Theta+\sqrt{(rs+2s\Theta+s^2\Theta^2)}}{1+s\theta+\sqrt{(rs+2s\theta+s^2\theta^2)}}\right]^{\frac{1}{m}\sqrt{\frac{A}{C}}}=e^{\frac{1}{m}\sqrt{\left(\frac{AR\sigma\alpha}{\kappa}\right)}x}, \qquad . \quad . \quad . \quad (39)$$

or

$$\left(1+\frac{r}{\theta}\right)\left(1+s\theta+\sqrt{rs+2s\theta+s^2\theta^2}\right)^{-\frac{1}{m}\sqrt{\frac{r}{s}}}=Ke^{\mu x}; \qquad [19]$$

and the function of θ on the left-hand side of this equation increases in geometrical progression for an arithmetical increase of x.

38. So far we have been practically exact; but it is now necessary to introduce several approximations. Notice that r is a large constant and s a small one; so that $\frac{\theta}{r}$ and $s\theta$ are small quantities whose squares may be neglected for all probable values of θ. Moreover observe that $\frac{1}{m}\sqrt{\frac{r}{s}}$, though it may be imaginary, is never large and is often fractional. Write, therefore, the denominator of the left-hand side of [19] in the following successively approximate forms,

$$\left[1+s\theta+\sqrt{rs}\sqrt{\left(1+\frac{2\theta}{r}\right)}\right]^{\frac{1}{m}\sqrt{\frac{r}{s}}}$$

$$\bumpeq\left[1+s\theta+\sqrt{rs}\left(1+\frac{\theta}{r}\right)\right]^{\frac{1}{m}\sqrt{\frac{r}{s}}}$$

$$=\left[1+\sqrt{rs}+\theta\left(s+\sqrt{\frac{s}{r}}\right)\right]^{\frac{1}{m}\sqrt{\frac{r}{s}}}$$

$$\bumpeq(1+\sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}}+\frac{1}{m}\theta(\sqrt{rs}+1)(1+\sqrt{rs})^{\frac{1}{m}\sqrt{\left(\frac{r}{s}\right)}-1}$$

$$=\frac{m+\theta}{m}(1+\sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}}.$$

Hence we have as the quantity which is to go in geometrical progression in equation [19]

$$\frac{1+\frac{r}{\theta}}{m+\theta}=\frac{(1+\sqrt{rs})^{\frac{1}{m}\sqrt{\frac{r}{s}}}}{m}.\mathrm{K}\,e^{\mu x}, \quad . \quad [19']$$

and we may conveniently write equation [14] to the curve of temperature down the rod in the simple approximate form

$$\theta.\frac{m+\theta}{r+\theta}=\Theta.\frac{m+\Theta}{r+\Theta}e^{-\mu x}, \quad . \quad . \quad . \quad (40)$$

where it is to be remembered, see equation (39), that

$$\mu^2=\frac{\mathrm{AR}\sigma\alpha}{m^2\kappa}\bumpeq^{*}\frac{mn\sigma\mathrm{R}\alpha}{m^2\kappa}=\frac{c_0\rho_0}{k_0}\mathrm{R}\alpha=\frac{c_0\rho_0}{k_0}.\mathrm{P}a^{v_0}\log a, \qquad [21]$$

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* The approximation in [21] consists in writing mn, the largest term of A, instead of A. It is unnecessary to work with this approximation; but it is useful as showing how naturally the various constants occur in μ.

and that

$$r=\frac{2\mathrm{A}}{\mathrm{B}}=\frac{6}{\alpha}\cdot\frac{\alpha mn+m+n-\frac{2}{\alpha}}{(2-\gamma)(m+n)+(1+\gamma)\left(\alpha mn+\frac{2}{\alpha}\right)}. \quad . \quad (41)$$

39. There ought now to be no difficulty in calculating the constant m from observed corresponding values of θ and x. The following method has occurred to me; but there may be better ones. Let five temperatures, θ_0, θ_1, θ_2, θ_3, θ_4, be observed along the rod at distances from the origin x, $x+\xi$, $x+2\xi$, $x+3\xi$, and $x+4\xi$. Let the quantity which goes in geometrical progression, see equations (40) or [19′], be written

$$\frac{1+\frac{r}{\theta}}{1+\frac{\theta}{m}};$$

and for shortness call the numerator $\phi(\theta)$ and the denominator $\psi(\theta)$. Then of course

$$\frac{\phi_1}{\psi_1}\cdot\frac{\phi_3}{\psi_3}=\left(\frac{\phi_2}{\psi_2}\right)^2,$$

whence

$$\frac{\phi_1\phi_3-\phi_2^2}{\phi_2^2}=\frac{\psi_1\psi_3-\psi_2^2}{\psi_2^2}.$$

Similarly

$$\frac{\phi_0\phi_4-\phi_2^2}{\phi_2^2}=\frac{\psi_0\psi_4-\psi_2^2}{\psi_2^2}.$$

Therefore

$$\frac{\phi_1\phi_3-\phi_2^2}{\phi_0\phi_4-\phi_2^2}=\frac{\psi_1\psi_3-\psi_2^2}{\psi_0\psi_4-\psi_2^2},$$

which, being interpreted, is

$$\frac{\frac{1}{\theta_1}+\frac{1}{\theta_3}-\frac{2}{\theta_2}+r\left(\frac{1}{\theta_1\theta_3}-\frac{1}{\theta_2^2}\right)}{\frac{1}{\theta_0}+\frac{1}{\theta_4}-\frac{2}{\theta_2}+r\left(\frac{1}{\theta_0\theta_4}-\frac{1}{\theta_2^2}\right)}=\frac{(\theta_1+\theta_3-2\theta_2)m+\theta_1\theta_3-\theta_2^2}{(\theta_0+\theta_4-2\theta_2)m+\theta_0\theta_4-\theta_2^2}, \quad . \quad . \quad . \quad . \quad (42)$$

the form of which we may abridge into

$$\frac{a_1+rb_1}{a_0+rb_0}=\frac{mc_1+d_1}{mc_0+d_0}$$

where the coefficients a, b, c, d are all known. This gives us

$$m=\frac{a_0d_1-a_1d_0+r(b_0d_1-b_1d_0)}{a_1c_0-a_0c_1+r(b_1c_0-b_0c_1)}. \quad . \quad . \quad . \quad (43)$$

40. Now looking at equation (41), we see that the value of r is $\frac{6}{\alpha}$, multiplied by a fraction which contains m indeed, but does not depend very much upon it; for, neglecting the small quantity γ, the fraction is

$$\frac{\alpha mn+m+n-\frac{2}{\alpha}}{\alpha mn+2(m+n)+\frac{2}{\alpha}};$$

and since m and n are both pretty large, the first term, which is the same in both numerator and denominator, is much the biggest, and accordingly the fraction is not very different from unity. It is likely to be greater than 1 when m is negative, and less than 1 when m is positive. Hence a first approximation to r is $\frac{6}{\alpha}$, or 780. The fact that r does not depend *much* upon m is apparent in the Table, § 34.

Making, then, a guess at r as 800 or so, we obtain from (43) a first approximation to m; and we can afterwards improve it by trial and error, so that the quantity $\theta\frac{m+\theta}{r+\theta}$ in equation (40) really does go in geometrical progression down the whole length of the rod—for instance, so that

$$\frac{1}{x}\log\frac{\Theta(m+\Theta)(r+\theta)}{\theta(m+\theta)(r+\Theta)}=\text{const}=\mu. \quad . \quad . \quad (44)$$

And then, having obtained the mean value of this constant μ, it is easy to calculate the absolute conductivity of the rod from [21] if one has determined R by experiments on cooling (see § 27). The *relative* thermometric conductivities of different metals at the temperature of the enclosure are simply inversely as μ^2 (see [21]).

41. The only experimental results already published which are even apparently at all suitable for applying the method to are those of Wiedemann and Franz; but it is impossible to get any results from these for reasons stated in § 28. It may, however, be interesting to see how far their numbers for an

iron rod in a vacuum will lend themselves to the equations now obtained, if m is assumed to be -700 and r to be 850, as in § 34.

The temperatures of the rod were read thermoelectrically, and stated in terms of galvanometer-deflections; and though a little Table is given showing what these deflections experimentally mean in Centigrade excess of temperature, the numbers actually used in their calculations are the deflections themselves, which are only roughly proportional to the temperature excesses. The following empirical relation between δ (the deflection) and θ (the excess of temperature) is deduced from their little comparison Table,

$$\theta = \tfrac{1}{4}\left(\delta + 2 - \frac{\delta}{10 - \frac{\delta}{100}}\right);$$

and this I have used to obtain the second column of the following Table:—

Iron (Wiedemann and Franz).

Galvanometer deflections. δ.	Excess of temperature in Centigrade degrees. θ.	$\log_{10}\left(\theta\,\frac{700-\theta}{850+\theta}\right)$ $= \log f(\theta)$.	$\frac{1}{n-1}\log\frac{f(\theta_1)}{f(\theta_n)}$ $= \mu\xi \log_{10} e$.	$\frac{f(\theta_{n-1})+f(\theta_{n+1})}{f(\theta_n)}$ $= 2\cosh \mu\xi$.
230	50·8	1·5637		
153½	34·5	1·4142	·1495[a]	2·094[b]
100½	22·9	1·2487	·1575	2·168
67½	15·8	1·0965	·1557	2·068
42	10·1	·9086	·1638	2·172
25·7	6·3	·7079	·1712	2·147
13·2	3·5	·4558	·1846	

[a] The mean of the first four numbers in this column is ·1566; therefore

$$\mu\xi = \cdot 3606.$$

[b] The mean of these numbers is 2·130; therefore

$$\mu\xi \bumpeq \sqrt{(2\cosh \mu\xi - 2)} = \cdot 3606.$$

The third column contains the logarithm of the quantity which ought to go in geometrical progression. The fourth column shows that this law of progress is moderately true for the first four numbers, but that the numbers which ought to be constant exhibit a decided increase towards the cooler end of the bar, probably because the bar was so short that the flow of heat along it extended through the point where $\theta=0$—which is contrary to our hypothesis (§§ 32 and 17) that $\frac{d\theta}{dx}$ shall vanish with θ. The distance ξ was 2·6 centimetres.

The next Table shows their results for silver tabulated in the same way, and on the assumption that $m=+1000$.

Silver (Wiedemann and Franz).

δ.	θ.	$\log\left(\theta\frac{1000+\theta}{700+\theta}\right)$ $=\log f(\theta)$.	$\frac{\log f(\theta_1)-\log f(\theta^n)}{n-1}$ $=\mu\xi\log e$.	$\frac{f(\theta_{n-1})+f(\theta_{n+1})}{f(\theta^n)}$ $=2\cosh\mu\xi$.
194	42·5	1·7758		
167	37·3	1·7200	·0558[a]	1·977[b]
142	32·0	1·6544	·0607	2·024
122	27·5	1·5893	·0621	2·018
104	23·5	1·5217	·0635	2·030
88½	20·2	1·4568	·0638	2·036
75½	17·7	1·3994	·0627	

[a] The mean of these numbers is ·0614; hence

$$\mu\xi=\frac{\cdot0614}{\cdot4343}=\cdot141.$$

[b] The mean of these is 2·017; hence

$$\mu\xi=\sqrt{\cdot017}=\cdot1304.$$

42. Although the whole investigation applies only to the case of a long rod, yet it seems extremely possible that something very like the proper equation to the curve of temperature down a short rod with given temperature at its two ends can be written down from equation (40) by the addition of another term to the right-hand side, thus,

$$\theta\frac{m+\theta}{r+\theta}=\mathrm{A}e^{\mu x}+\mathrm{B}e^{-\mu x}, \quad . \quad . \quad . \quad . \quad (45)$$

just as the ordinary equation for an infinite rod

$$\theta=\Theta e^{-\mu x}$$

becomes

$$\theta=\mathrm{A}e^{\mu x}+\mathrm{B}e^{-\mu x}$$

for a short one. If so, the conductivity-constant μ and the variation-constant m would be best determined from the relation

$$\frac{\theta_1\dfrac{m+\theta_1}{r+\theta_1}+\theta_3\dfrac{m+\theta_3}{r+\theta_3}}{\theta_2\dfrac{m+\theta_2}{r+\theta_2}}=\text{const}=2\cosh\mu\xi \quad . \quad . \qquad [25]$$

by some such process as is given in §§ 23–25.

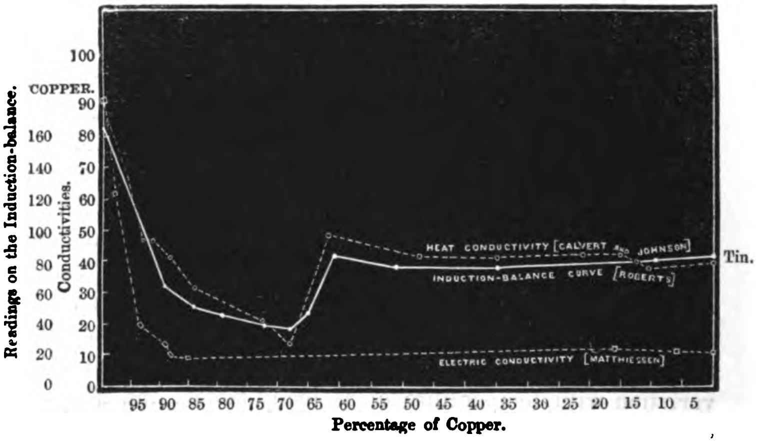
The extent of the constancy of this quotient with Wiedemann's numbers is therefore exhibited in the last column of the preceding tables; and the value of $\mu\xi$ is obtained from this column in order to compare it with that obtained from the column preceding. In the case of iron the two values accidentally agree exactly.

43. As regards the concluding section of the former paper (the suggestions for future experiment), I have seen no reason to modify it in any way. I still think the rods should be examined by a thermoelectric compensation method (*i. e.* with the use of a bridge wire and slider, and with fixed thermoelectric joints on the rod) after their surfaces have been uniformly coated with stove black-lead. The exhaustion of the chamber containing the rod should proceed to the "neutral-point" observed by Mr. Crookes, and employed by Mr. Poynting in his delicate weighing experiment (Proc. Roy. Soc.), "when convection currents have ceased and the radiometer effect has not yet begun." It is probable that at such a point (if it really exists, as I suppose it does) Dulong and Petit's law of cooling, assumed all through this paper, would hold with perfect accuracy; and I think that with very careful observations of temperature over a sufficient range on long thin rods, there would be no insuperable obstacle to obtaining rapidly the absolute conductivity and the coefficient of its variation with temperature of as many metals and alloys as one pleased.

I have taken considerable care; and I hope that all the calculations in the present paper are free from error; but they are rather long and unsymmetrical and therefore tiresome, and I have not ventured to ask any one to read over the proof for this very reason.

XXII. *On an Analogy between the Conductivity for Heat and the Induction-Balance Effect of Copper-Tin Alloys.* By W. Chandler Roberts, *F.R.S.*[*]

In a paper submitted to the Society in June last, I pointed out that the results obtained when copper-tin alloys are examined by the aid of the induction-balance does not correspond with Matthiessen's determinations of the electric conductivities of the same alloys. The nature of the induction-curve remained therefore more or less obscure; but I have recently observed a remarkable resemblance between my own results[†] and those given by Calvert and Johnson[‡] for the conductivity of heat, which had hitherto escaped me, as the authors did not plot their figures. The following diagram shows the relation of the curve published by me in July last with those of Calvert and Johnson and Matthiessen respectively, which I find for the first time placed in juxtaposition in a valuable report on the copper-tin alloys recently issued by the United-States Government[§].



Note.—The curves for the conductivity of heat and electricity are taken from Plate xiv. of the Report on Copper-Tin Alloys, above referred to. The latter curve should, however, be moved slightly to the left, as Matthiessen's numbers represent *volumes*, and not weights, per cent. of copper.

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[*] Read November 8th.

[†] Phil. Mag. (5) vol. viii. p. 57, 1879.

[‡] Phil. Trans. vol. cxlviii. 1858, p. 349.

[§] Report made under the Direction of the Committee on Metallic Alloys. Washington, 1879.

The earlier results, which were obtained by observations on blocks of alloy 18 millims. square and 7 millims. thick, have since been verified (except in the case of pure copper, which as originally given is too low) by a series of disks 24 millims. in diameter and 3 millims. thick, formed in the lathe, or, in the case of the very brittle ones, by filing.

The close agreement between the conductivity for heat and electricity has, as is well known, been pointed out by Wiedemann and Franz*; but as the similarity has hitherto, I believe, only been observed in isolated cases, the above results on an extended series of alloys may be of interest.

Calvert and Johnson claim a high degree of accuracy for their results; but they were subjected to adverse criticism, as Wiedemann † objected to the apparatus they adopted and to the small size of the bars employed. It is therefore the more important to point to the almost absolute identity of the two curves representing phenomena of which the manifestations are so different.

The respective values of the alloys were ascertained by placing the metal to be examined on one side of the balance and by superposing a graduated wedge-shaped scale of zinc over the opposing coil, as has already been described by Prof. Hughes ‡. The figures given, can however, only be considered to represent the general relation among themselves of the various alloys in the series, as the zinc scale does not give absolute values.

The induction-balance curve, as I have previously pointed out, bears an evident relation to the curve representing the density of the same alloys; but its divergence from Matthiessen's curve § of conductivity is singular. This may perhaps be explained by the fact that Matthiessen does not appear to have examined any alloy between those which contain respectively 16·4 and 85·1 volumes per cent. of copper, probably because the alloys between these points are too brittle to permit their being formed into wire. It may be noted, however, that the curve of the tin-gold alloys, which belong to the same group as the copper-tin, is given in his well-known paper[*] in 1860, and does not show an unbroken line in the horizontal portion. Certain intermediate alloys, in the form of rods, are now being examined by Dr. Lodge, whose results will doubtless clear up the point.

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* Poggendorff's *Annalen*, vol. lxxxix. [1853], pp. 497–531.

† *Ibid.* vol. cviii. [1859], pp. 393–407.

‡ Phil. Mag. (5) vol. viii. p. 50.

§ Brit. Assoc. Report, 1863, p. 37, and Chem. Soc. Journ. 1867, p. 212.

The alloys which occupy the critical points of the induction-balance curve are very interesting. They may be represented by the formulæ $SnCu_3$ and $SnCu_4$ respectively; and although they only vary by a single equivalent, or by 6·49 per cent. of copper, their structure and appearance differ widely. The latter, $SnCu_4$, is a speculum-metal. It has a large conchoidal fracture and a yellow-grey tint. $SnCu_3$, on the other hand, has a blue-grey colour and a coarse surface of interrupted crystalline planes. By successive additions of copper, this alloy seems to pass into the other without any sharp break.

Possibly both alloys are chemical combinations; and the difference of their ordinates probably marks a different allotropic state. For further information on such questions, however, we may look with confidence to Prof. Hughes's beautiful and simple instrument, which will also help us to determine whether the relation between conductivity for heat and electricity is really as exact as it has hitherto been supposed to be.

XXIII. *Note on a Determination of the Specific Electrical Resistance of certain Copper-Tin Alloys.* *By* Oliver J. Lodge, *D.Sc., Assistant Professor of Physics in University College, London.*

On the 5th of November I received from Mr. Chandler Roberts three rods of certain very brittle copper-tin alloys which he had with some difficulty cast, in order that I might determine their specific resistance per unit of volume by ordinary processes, because the readings which the induction-balance gave for disks of these three metals indicated that their conductivities did not by any means agree with Matthiessen's curve of electric conductivity for copper-tin alloys, though, on the other hand, they did agree with some severely criticised and discarded experiments of Messrs. Calvert and Johnson on the conductivities of these same alloys for *heat.*

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[*] Phil. Trans. vol. cl. p. 161.

The rods, which are hereafter spoken of as A, B, and C, were about 9 millimetres thick and from 30 to 40 centimetres long. The method which I employed to determine their conductivity was scarcely altered from that which was described by Prof. Foster and myself in this Journal in 1875*; and the results which I have obtained I believe to be somewhat accurate. It was not worth while to aim at any very *excessive* accuracy, though the method is quite capable of it (by a calibration of the bridge-wire), because the temperature of the alloys was a little indefinite, being simply that of the room.

The three rods were laid end to end in a groove in a long piece of wood so that they just projected above its surface; and their ends were then screwed up into secure contact with pads of tinfoil between. The resistance of the contacts is not of the slightest consequence, provided it remains constant and is moderately small, all measurements being made within the length of the rod itself.

A bridge-slider was arranged so as to make contact with one of the rods at a point whose position could be read by a millimetre-scale fixed at the proper level. The length of the portion of the rod examined could be ascertained within the tenth of a millimetre; and the resistance of this portion could also be read with great accuracy. The least satisfactory part of the measurement is the gauging of the diameter of the rod; for they are not perfectly cylindrical, and the *average* cross section has to be taken. I would have had the rods turned; only two of them, viz. B and C, are far too brittle to be touched with any tool.

The observation of resistance consisted in placing the slider at a definite point on one of the rods, and feeling about on the bridge-wire with another slider for a point of the same potential. The slider on the rod was then shifted to another part of the same rod and the bridge-slider shifted correspondingly. The shifts on the bridge-wire (German silver) were of course rather small; but they could be read to the tenth of a millimetre; and, moreover, the resistances in the circuit were so adjusted that any step along the rods corresponded to a step of 3·62 times the resistance along the bridge-wire. It is needless to say that a weak battery and a reflecting galvanometer were used, and that the contacts were only made momentarily. As an example of the error to be expected I may quote the following.

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* Foster and Lodge, "On the Flow of Electricity in a Plane.—Part II.," Phil. Mag. December 1875.

Two different steps along rod A gave, as the step on the bridge-wire corresponding to the resistance of 1 centimetre length of the rod A, the numbers

·0328 and ·0325 centim.

Two different steps along rod B gave

·0269 and ·0270 centim.

Different steps along rod C, which was a trifle less uniform, gave

·123, ·121, ·1177, ·1173, ·1185, ·1171 centim.

As the step ·1185 was deduced from the entire length of the rod, I have taken this as the mean. The steps which gave ·123 and ·121 were short ones; and their probable error is therefore greater than that of the others. Moreover they were made at the thinner end of the not quite cylindrical rod. As regards the mean cross section of the rods, it was estimated by taking a number of readings of the diameters in all parts of the rods by means of a micrometer-gauge reading to the hundredth of a millimetre.

The mean diameter of rod A was ·879 centim.
„ „ „ B „ ·873 „
„ „ „ C „ ·869 „

The rods had been cast in the same mould; but C had contracted more than the others.

On the 7th of November Mr. Roberts sent me three more rods, which I labelled D, E, F, and then determined their resistance in the same way.

The mean diameter of these rods, which were perfectly strong and might have been drawn into wire, was

D	·880 centim.
E	·862 „
F	·872 „

These had had their surfaces filed after casting.

To get the value of the bridge-wire-readings in absolute measure, a half-ohm coil of German-silver wire at the temperature of the room (*i. e.* of the bridge-wire) was inserted in the place of the rods, and readings taken of the steps corresponding to it. The scale of magnification was of course again observed. Instead of being about 3·6, it was now about $\frac{1}{3}$.

The result of the whole series of measurements may be summed up in the following Table:—

Specific Resistances of the six Copper-Tin Alloys in square centimetres per second.

A	12960
B	10960
C	47660
D	11830
E	17090
F	15270

I consider these accurate to the second figure, the third figure is probably affected by experimental errors.

If we choose to express the results in terms of conductivity, we have the following:—

Conductivity of a Centimetre Cube of the six Alloys in B.A. units.

A	77100
B	91200
C	21000
D	84500
E	58500
F	65500

The specific resistance of the alloy C is thus four times that of B or D.

The composition of the alloys by weight, Mr. Roberts informs me, is as follows:—

A	19·2 per cent. of copper	80·8	tin.
B	61·8 ,, ,,	38·2	,,
C	68·3 ,, ,,	31·7	,,
D	0·0 ,, ,,	100·0	,,
E	87·4 ,, ,,	12·6	,,
F	90·3 ,, ,,	9·7	,,

Some of these alloys were examined by Matthiessen, viz. A, D, E, and F; and the conductivity of a wire 1 metre long and 1 millimetre thick is stated by him to be, in terms of the B.A. unit,

of the metal A	6·0
,, D (pure tin)	5·9
,, E	4·2
,, F	5·9

To reduce these numbers from a wire to a centimetre cube we must multiply them by $\frac{40000}{\pi}$, when they become

for A	76400
„ D	75100
„ E	53500
„ F	75100

of which, therefore, A and E agree tolerably with my measurements on the rods.

Matthiessen also measured the conductivity of pure copper, his number when reduced being

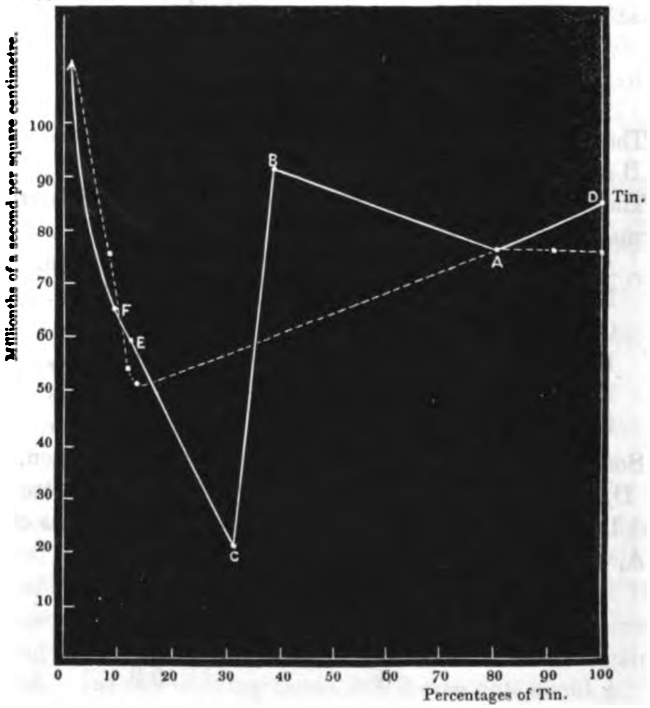
Copper	610000

or about eight times that of pure tin.

The results may be plotted in order to compare them more readily with the readings given by the induction-balance. The abscissæ represent percentages of tin; the ordinates represent conductivities divided by 1000.

Specific Conductivities of Copper-Tin Alloys.

Copper is at a height 610.



The continuous broken line joins together my six points of observation. The dotted line is Matthiessen's curve of conductivity on the same scale, viz. absolute measure, with some of his points of observation marked on it; but no alloys between somewhere about E and A seem to have been actually examined by him.

The position of pure copper (from Matthiessen's measurement) cannot be shown in the diagram: the conductivity decreases at such a tremendous rate with the slightest percentage of tin that the ordinate for pure copper is 600 scale-divisions, whereas the greatest height shown in the diagram, that of the point B, is only about 90.

It is unnecessary to call attention to the extraordinary behaviour of the alloy C containing 31·7 per cent. of tin (that is, the alloy $SnCu_4$), or to point out how easily it might have been supposed to be a distinct metal.

XXIV. *On a Suggestion as to the Constitution of Chlorine, offered by the Dynamical Theory of Gases.* *By* A. W. Rücker, *M.A., Professor of Physics in the Yorkshire College, Leeds**.

If a gas of density δ consists of molecules each of which possesses m degrees of freedom, and if also the intermolecular forces are negligible, the specific heats at constant pressure (c_p) and at constant volume (c_v) are connected by the two well-known equations

$$(c_p - c_v)\delta = \cdot 0694, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{c_p}{c_v} = 1 + \frac{2}{m+e}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where e is a quantity which depends upon the potential energy of a molecule. Hence, if c_p is given by experiment, c_v can be calculated from the first of these equations; and then $m+e$ is known from the second.

The accuracy of the value of $m+e$ thus deduced will depend upon that of c_p, and on the legitimacy of the application of the two equations to the gas or vapour under consideration.

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* Read November 22nd.

With respect to the first of these points, it may be remarked that E. Wiedemann has recently (Pogg. *Ann.* Bd. clvii. p. 1, 1876, and Wied. *Ann.* Bd. ii. p. 195, 1877) determined the specific heats at constant pressure of 14 out of the 35 gases and vapours studied by Regnault. The difference between the results of the two investigators amounts in two cases only (ethylene and ammonia) to 6 per cent.; in three cases it is about 5 per cent., and in all the others less. Thus even on the assumption that the later experiments are absolutely correct, it follows that Regnault's numbers may be trusted to 6 per cent. His results, however, can only be taken as true for the particular temperatures at which the experiments were made, as Wiedemann shows that in all but the most perfect gases the specific heat at constant pressure varies considerably with the temperature.

More recently still, Wüllner (Wied. *Ann.* Bd. iv. p. 321, 1878), using Kundt and Warburg's method, has determined the ratio of the specific heats of air, carbonic oxide, carbonic acid, nitrous acid, ethylene, and ammonia at 0° and 100° C. He finds that for gases which obey Boyle's law, c_p and c_v are constant, but that in the case of less perfectly gaseous bodies they increase with the temperature. The difference between them, however, is always very approximately constant and equal to the theoretical number—thus justifying the application of the first of the above equations to imperfect gases, and proving that the observed increase in the specific heats is due to work done within the molecules, and not against the intermolecular forces, which must therefore be negligible.

On the whole, then, the result of these researches is to show that $m+e$ can be calculated very approximately from the above equations if c_p is given, and that Regnault's values of this quantity are probably trustworthy to 6 per cent.

One of the chief difficulties of the thermodynamic theory of gases has been to attribute to m and e values which would at once lead to the observed ratios of c_p to c_v, and satisfy any rational supposition as to the interior mechanism of a molecule. Kundt and Warburg proved that for mercury vapour $\frac{c_p}{c_v}=1{\cdot}666$, which is consistent only with the supposition that the atoms of that substance are smooth rigid spheres. Boltzmann (Pogg. *Ann.* Bd. clx. p.175, 1877) and Bosanquet (Phil. Mag. April 1877) have since drawn attention to the fact that, for a smooth rigid surface of revolution, $m=5$ and $e=0$, which would make $\frac{c_p}{c_v}=1{\cdot}4$. The fact, therefore, that this number agrees very closely with those given by experiment for a large number of gases (air, O, N, H, CO, and NO) would be accounted for by supposing their molecules to be surfaces of revolution. This condition would be fulfilled, as is pointed out by Mr. Bosanquet, by two spheres rigidly united, and would thus accord well with our conception of the atomic constitution of the above gases. It would perhaps be better to regard the spheres not as rigidly united, but as bound together by forces which prevent the separation of their surfaces, while leaving them otherwise free to move. The required five degrees of freedom would thus be obtained, and the hypothesis would better coincide with the supposition of the union of two *smooth* spheres to form the ultimate particles of the gases.

For a discussion of the difficulties offered to any such theory by the spectroscope I must refer to the above papers; my present purpose is to point out an interesting fact connected with its application to chlorine and its compounds.

The maximum number of degrees of freedom which a molecule composed of n smooth rigid spheres could possess would be $3n$; but the forces in play between the spheres might, as in the case of air and the other gases above referred to, reduce the number to much below this amount. Thus the value of $m+e$ could not exceed, but might be less than $3n+e$. For gases in which the molecule consists of two atoms, $e=0$; but in the cases of more complex combinations we can say only that, *cæteris paribus*, we should expect that its value would increase with the number of atoms in the molecule. Bearing these facts in mind, the following Tables lead to a curious result. In the first column of each are placed the symbols of the substances referred to. In the second columns are the ratios of the specific heats, deduced (except in the case of mercury) from Regnault's experiments by the use of equation (1). In the third are the values of $m+e$ (omitting fractions) deduced by equation (2). In the fourth are the values of $3n$, where n is the number of atoms of which the molecule is composed.

TABLE I.

I.	II.	III.	IV.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.
Hg …………	1·666	3	3
Air …………	1·413	5	6
O_2 …………	1·403	5	6
N_2 …………	1·409	5	6
H_2 …………	1·417	5	6
NO …………	1·403	5	6
CO …………	1·416	5	6
N_2O ………	1·243	8	9
H_2O ………	1·302	7	9
H_2S ………	1·335	6	9
CO_2 ………	1·265	8	9
SO_2 …………	1·276	7	9
CS_2 …………	1·198	10	9
NH_3 ………	1·300	7	12
CH_4 ………	1·290	7	15
C_2H_4 ………	1·144	14	18
C_2H_6O ……	1·110	18	27
$C_4H_{10}O$ ……	1·059	34	45
$C_4H_{10}S$ ……	1·059	34	45
C_3H_5N ……	1·283	7	27
$C_4H_8O_2$ ……	1·060	33	42
C_3H_6O ……	1·091	22	30
C_6H_6 ………	1·072	28	36
$C_{10}H_{16}$:……	1·031	64	78

TABLE II.

I.	II.	III.	IV.	V.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.	
Cl_2 …………	1·286	7	6	18
HCl ………	1·408	5	6	12
C_2H_5Cl……	1·131	15	24	30
$CHCl_3$ ……	1·118	17	15	33
$C_2H_4Cl_2$ …	1·097	21	24	36
PCl_3 ………	1·122	16	12	30
$AsCl_3$ ………	1·111	18	12	30
$SiCl_4$ ………	1·098	20	15	39
$SnCl_4$ ………	1·092	22	15	39
$TiCl_4$ ………	1·089	22	15	39

The first Table contains a number of simple and more or less complex compound gases and vapours; the second is confined to chlorine and its compounds alone. The difference between the two is most marked. In the first the value of $m + e$ is for every substance (with one exception) less than $3n$, or than the maximum possible value of m. In the second the reverse statement holds good in more than two thirds of the whole number of cases.

This difference can hardly be accidental; nor can it be explained by an error of 6 per cent. in Regnault's experiments. It might be accounted for by supposing that in the case of chlorine e is abnormally large—and that this gas differs from others in which the molecule is built up of two spheres, in that the spheres are not necessarily in contact, and are probably therefore less firmly united.

Another supposition, however, would meet the case equally well, viz. that n has been taken too small, that the symbol Cl_2 is incorrect, and that the atoms of chlorine, and therefore the molecules of its compounds, contain a larger number of subatoms or atoms than has been supposed. It need hardly be added that this supposition fits in most satisfactorily with the results of the recent researches of Prof. Victor Meyer on the vapour-density of chlorine; and in the fifth column of Table II. are given the values of $3n$ calculated on the assumption that throughout the first column we ought to write Cl_3 for Cl. Hydrochloric acid now offers a difficulty, as a body composed of four spheres could only possess so small a number of degrees of freedom as five if the spheres were rigidly connected with their centres in one straight line. With this exception, however, columns III. and V. of Table II. now present differences of the same sign and order as those in the corresponding columns in Table I.

The number of degrees of freedom attributed to each substance may perhaps be wrong by one in some of the simpler bodies, and by rather larger numbers in some of the more complex; but the general character of the Tables is probably beyond the reach of any such changes. An error *e. g.* of 6 per cent. in the specific heat at constant pressure of the tetrachlorides would only reduce the number of their degrees of freedom by one. Much greater alterations would be introduced by taking the specific heats at other temperatures than those at which they were determined by Regnault. Chloroform is the only compound of chlorine of which the law of the variation of the specific heat with the temperature was studied by Wiedemann; and using his results, I find that at 0° C. the number of degrees of freedom of this substance would be one less instead of two greater than 15.

In spite, however, of the uncertainty thus introduced, the comparison of the two Tables is sufficiently suggestive to induce me to lay them before the Physical Society. The fact that the application of the theory of gases to the specific heats of a large number of substances, determined as far as might be under similar circumstances, leads to the alternatives that the atoms of which a molecule of free chlorine is composed are either less strongly united or are more numerous than in the case of other elements, is not unimportant.

It remains to add that the ratios of the specific heats of bromine and of the only one of its compounds which has been studied agree with those of chlorine and the corresponding chlorine compound.

TABLE III.

I.	II.	III.	IV.	V.
	$\frac{c_p}{c_v}$.	$m+e$.	$3n$.	
Br_2	1·302	7	6	18
C_2H_5Br ...	1·114	18	24	30

Note.—Since the above was written, the conclusions at first drawn from Prof. V. Meyer's research have been questioned (Chem. News, Nov. 21, 1879, p. 244) on the ground that experiments made by Seelheim, of Utrecht, indicate the possibility of the formation of a volatile chloride of platinum at high temperatures. However this may be, the cause of the anomalous specific heats of chlorine and its compounds remains to be explained; and the above statement of the alternative suppositions to which the theory discussed leads may not be uninteresting, even if it should be proved that one of them lacks the support which, at the time of writing, Prof. V. Meyer's research was supposed to afford it.

XXV. *On the Graduation of the Sonometer.* *By* J. H. Poynting, *M.A., Fellow of Trinity College, Cambridge**.

It seems likely that such valuable results will be obtained by means of Professor Hughes's sonometer, that it is desirable that some method should be employed to turn its at present arbitrary readings into absolute measure, so that, for instance, the induced currents caused by different metals in the induction-balance may be measured and compared with each other.

In Maxwell's 'Electricity,' vol. ii. chap. xiv., the general formula is given for the coefficient of induction of one circular circuit on another. Adapting this to the case where two equal circular circuits are on the same axis at a distance apart greater than the radius of the coils, the following formula is obtained.

Let a = distance between centres,
b = radius of either circle,
c = distance of either circumference from centre of other,
M = coefficient of induction.

Then

$$\mathrm{M} = -\frac{4\pi^2 b^4}{c^3}\left\{\frac{1}{2} - \frac{3}{4}\frac{b^2}{c^2} + \frac{15}{8}\frac{b^4}{c^4} - \frac{35}{8}\frac{b^6}{c^6} + \frac{2835}{256}\frac{b^8}{c^8} - \&c. \right. \quad (1)$$

or

$$= -\frac{4\pi^2 b^4}{a^3}\left\{\frac{1}{2} - \frac{3}{2}\frac{b^2}{a^2} + \frac{75}{16}\frac{b^4}{a^4} - \frac{490}{32}\frac{b^6}{a^6} + \frac{24570}{256}\frac{b^8}{a^8} - \&c. \right. \quad (2)$$

Of these the latter uses directly the distance between the centres, the observed quantity—but is not nearly so convergent as the former, in which c may be at once deduced from $c = \sqrt{a^2 + b^2}$.

To obtain formulæ which might be strictly applied to the sonometer, we should have to consider the more general case of two coils of unequal radii b and β, for which I have found the formula corresponding to (2), viz.

$$\mathrm{M} = \frac{4\pi b^2\beta^2}{a^3}\left(\frac{1}{2} - \frac{3}{4}\frac{b^2+\beta^2}{a^2} + \frac{15}{16}\frac{b^4+3b^2\beta^2+\beta^4}{a^4} \right.$$
$$\left. - \frac{35}{32}\frac{b^6+6b^4\beta^2+6b^2\beta^4+\beta^6}{a^6} + \&c. \right. \quad (3)$$

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* Read December 13th, 1879.

We should then have to take the finite integrals of each term between the limiting values of b and β. But this would be exceedingly complicated and would require a knowledge of all the details of construction; and we may at least get a first approximation to the true result by replacing the coils by a single one of a radius intermediate between the greatest and least radii.

In Prof. Hughes's paper (Phil. Mag. July 1879) he gives the internal and external radii of his coils as 15 millims. and 27·5 millims. respectively. I have considered, then, that 25 millims. will give results not very far from the truth; and as it makes the calculations considerably easier, I have taken that as the value of b and applied the formulæ to the numbers given in the paper. The resultant current in the middle coil was zero when it was distant 47 millims. from one end and 200 from the other. This enables us to find the ratio between the number of turns in the two ends at least sufficiently nearly to apply to some of the results.

Let M_1 be the coefficient of induction of the larger coil on the movable one, M_2 that of the smaller, the former having m turns, the latter n. When the movable coil was 200 millims. from the large and 47 millims. from the small coil, since there was no induced current,

$$mM_1 = nM_2.$$

Applying formula (1), we have c for the larger coil

$$=\sqrt{200^2+25^2}=201{\cdot}5,$$

and for the smaller coil

$$c=\sqrt{47^2+25^2}=53{\cdot}2,$$

b being the same for both. Then

$$\frac{m}{(201{\cdot}5)^3}\left\{\frac{1}{2}-\frac{3}{4}\left(\frac{25}{201{\cdot}5}\right)^2+\frac{15}{8}\left(\frac{25}{201{\cdot}5}\right)^4-\&c.\right\}$$

$$=\frac{n}{(53{\cdot}2)^3}\left\{\frac{1}{2}-\frac{3}{4}\left(\frac{25}{53{\cdot}2}\right)^2+\frac{15}{8}\left(\frac{25}{53{\cdot}2}\right)^4-\frac{35}{8}\left(\frac{25}{53{\cdot}2}\right)^6\right.$$

$$\left.+\frac{2835}{256}\left(\frac{25}{53{\cdot}2}\right)^8-\&c.\right.$$

Multiplying each side by 2 and finding the successive terms,

$$m \times \frac{122}{10^9} \{1 - {\cdot}02308 + {\cdot}00088 - \&c.\}$$

$$= n \times \frac{6645}{10^9} \{1 - {\cdot}33123 + {\cdot}18286 - {\cdot}09422 + {\cdot}02633,$$

or

$$\frac{m}{n} = 43{\cdot}6.$$

I have applied the formula to the results for various metals given by Prof. Hughes in a table in his paper. In the table below, in the first column are Prof. Hughes's numbers, *i. e.* distances from the point of no induction. In the second are numbers proportional to $mM_1 - nM_2$; where M_1, M_2 are the coefficients of induction of two simple coils calculated on the above hypothesis, m and n the number of turns in the two respectively. In the third column are the resistances for bars of the metal 100 millims. long and 1 millim. in diameter (Jenkin, p. 249). In the last column are the products of the numbers in the two preceding columns.

Metal.	Distance from point of no induction.	$mM_1 - nM_2$, proportional to	R.	$(mM_1 - nM_2)$R.
Silver	125	178	·21	37·4
Gold	117	135	·27	36·5
Aluminium	112	116	·375	43·5
Copper	100	84	·21	17·6
Zinc	80	50·1	·72	36·1
Tin	74	44·6	1·70	75·8
Iron	45	22·46	1·25	28·1
Lead	38	18·87	2·5	47·2
Antimony	35	17·35	4·5	78·1
Bismuth	10	5·75	16·8	96·6

Mercury has been omitted, as it gives a very much higher value than any of the others. Were the induced currents in the induction-balance proportional to the resistances given in the table, the numbers in the last column would of course be all the same. The deviations from equality are far greater than could be accounted for by errors in the approximations I have adopted, especially for the metals not at the beginning or end of the list. Hence we are driven to conclude, either that the resistances of the metals given in the tables are not the same as the resistances of the metals used by Prof. Hughes, or that the induced current is not proportional to the conductivity of the metal.

It should be noticed that the method of measuring currents by the sonometer assumes that the telephone integrates, as it were, the current; *i. e.* the loudness of the sound depends only on the total current, not on the time during which the current is passing, provided that the time be very short. I do not know whether this point has been investigated; but if not, it would probably be easy to examine it by means of the sonometer. It would be advisable to modify the instrument in such a way that the formulæ might be more easily employed, and that the approximations might be nearer to the truth.

The formulæ used in this paper may be obtained as follows, the method being adapted from that given in Maxwell.

The potential of a circular unit current at any point is the same as that of a magnetic shell of unit strength bounded by the circuit. This, again, is the same as the attraction of a thin plate of matter of unit surface-density in a direction perpendicular to the plane of the plate. If ω be the attraction of a plate of radius b, at a point distant b from the plate along its axis,

$$\omega = 2\pi\left(1 - \frac{1}{\sqrt{1+\frac{b^2}{c^3}}}\right)$$

$$= 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2} - \frac{1.3}{2.4}\frac{b}{c^4} + \frac{1.3.5}{2.4.6}\frac{b^6}{c^6} - \&c.\right\}.$$

If we introduce zonal harmonics as coefficients, this becomes

$$\omega = 2\pi\left\{\frac{1}{2}\frac{b^2}{c^2}P_1 - \frac{1.3}{2.4}\frac{b^4}{c^4}P_3 + \frac{1.3.5}{2.4.6}\frac{b^6}{c^6}P_5 - \&c.\right\}.$$

This is now the potential at any point in space where $b < c$.

If there be a second circular circuit of radius β on the same axis, we may suppose it replaced by a magnetic shell bounded by the current and lying on the sphere, with centre at the centre of the first current, the radius of the sphere being c.

This shell may be considered to consist of two layers of matter of equal and opposite densities, μ and $-\mu$, at distances c and $c+dc$ from the centre. The potential on the second layer is

$$\iint \mu\omega dS,$$

where the integration is taken over the shell. The potential on the second layer is

$$-\iint \mu\left(\omega+\frac{d\omega}{dc}dc\right)dS,$$

the sum being

$$-\iint \mu\frac{d\omega}{dc}dc\,dS\,;$$

but since the strength $=1$, $\mu dc=1$, and we have the mutual potential

$$M=-\iint\frac{d\omega}{dc}dS.$$

Replacing the element dS by $c^2 d\mu\, d\phi$, the limits will be for ϕ from 0 to 2π, and for μ from 1 to μ.

Integrating with respect to ϕ, and remembering that c is constant in integrating for μ, we have

$$\begin{aligned} M &= 2\pi c^2\int_\mu^1\frac{d\omega}{dc}d\mu \\ &= -4\pi^2c^2\left\{\frac{b^2}{c^3}\int_\mu^1 P_1 d\mu-\frac{1\,.\,3}{2\,.\,4}\,.\,4\frac{b^4}{c^5}\int_\mu^1 P_3 d\mu+\&c.\right\}. \end{aligned}$$

But we have the relation for zonal harmonics,

$$\int_\mu^1 P_n d\mu=\frac{1-\mu^2}{n(n+1)}\frac{dP_n}{d\mu}.$$

Substituting, we obtain

$$\begin{aligned} M=-4\pi^2(1-\mu^2)\Bigg\{&\frac{b^2}{2c}\frac{dP_1}{d\mu}-\frac{1\,.\,3}{2\,.\,4}\frac{b^4}{3c^3}\frac{dP_3}{d\mu} \\ &+\frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}\frac{b^6}{5c^5}\frac{dP_5}{d\mu}-\&c.\Bigg\}. \end{aligned}$$

The following are the values for the coefficients (Ferrer's 'Spherical Harmonics,' p. 23), both in terms of μ and when we substitute $\mu^2=1-\frac{\beta^2}{c^2}$:—

$$\frac{dP_1}{d\mu}=1,$$

$$\frac{dP_3}{d\mu}=\frac{3}{2}(5\mu^2-1)=\frac{3}{2}\left(4-5\frac{\beta^2}{c^2}\right),$$

$$\frac{dP_5}{d\mu}=\frac{15}{8}(21\mu^4-14\mu^2+1)=\frac{15}{8}\left(8-28\frac{\beta^2}{c^2}+21\frac{b^4}{c^4}\right),$$

$$\frac{dP_7}{d\mu}=\frac{3003\mu^6-3465\mu^4+945\mu^2-35}{16}=\frac{448-3024\frac{\beta^2}{c^2}+\&c.}{16},$$

$$\frac{dP_9}{d\mu}=\frac{109395\mu^8-180180\mu^6+90090\mu^4-13860\mu^3+315}{128}$$

$$=\frac{5760+\&c.}{128}.$$

Substituting these values and putting $c^2=a^2+\beta^2$,

$$\therefore \text{M}=-4\pi\frac{\beta^2b^2}{a^3}\left\{\frac{1}{2}-\frac{3}{4}\frac{b^2+\beta^2}{a^2}+\frac{15}{16}\frac{b^4+3b^2\beta^2+\beta^4}{a^4}\right.$$
$$\left.-\frac{35}{32}\frac{b^6+6b^4\beta^2+6b^2\beta^4+\beta^2}{a^6}+\&c.\right\},$$

The more useful form is obtained by retaining c. If we take the two circles of equal radius (i. e. $b=\beta$), we obtain

$$\text{M}=-4\pi^2\frac{b^4}{c^3}\left\{\frac{1}{2}-\frac{3}{4}\frac{b^2}{c^2}+\frac{15}{8}\frac{b^4}{c^4}-\frac{35}{8}\frac{b^6}{c^6}+\frac{2835}{256}\frac{b^8}{c^8}-\&c.\right\}.$$

XXVI. *On a new Form of Resistance-Balance adapted for comparing Standard Coils.* By J. A. Fleming, *D.Sc.* (*Univ. Lond.*), *Scholar of St. John's College, Cambridge*[*].

[Plate XIX.]

1. THE British Association Committee on electrical standards concluded their valuable labours on the unit of resistance by constructing copies of the selected standard. Certain of these coils, some fourteen in number, are at present preserved in the Cavendish Laboratory, Cambridge. It is

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[*] Read December 13th, 1879.

important that these coils, which consist of wires of various alloys, should be from time to time carefully compared together in order to determine whether the ratio of their resistances at definite temperatures remains the same*. Observations ought also at the same time to be made of the temperatures at which they agree, and also of their coefficients of variation of resistance with the temperature. In using for the purpose the ordinary form of divided-metre bridge, several objections present themselves which render it a tedious process to determine accurately the difference in the resistance of two coils at different temperatures, and hence to deduce their variation-coefficients. It seemed, on consideration, that a modification of the usual form of Wheatstone's bridge would render these processes more expeditious and at the same time more accurate. It is the object of the present paper to describe a form of resistance-balance which has been recently constructed for the Cavendish Laboratory, and which experience shows to have several decided advantages over the old form.

2. *Description of the Resistance-balance.*—A circular disk of mahogany 18 inches in diameter and about 1 inch thick (f) (Pl. XIX. figs. 1 and 2) stands upon three short feet L. Upon this, and concentric with it, is screwed down a disk of ebonite 14 inches in diameter and $\frac{3}{4}$ of an inch thick (e). This ebonite disk has a semicircular groove turned in its circumference. The circular wooden base extends on one side into a narrow rectangle (j) 4 inches wide and of the same thickness as the disk. To this are connected two other rectangular pieces (h, i,) which are joined together by slotted brass bars (y, see fig. 2) underneath, in such a manner as to permit the two intervals to be made wider or narrower at pleasure. This promontory is of wood, of the same material and thickness as the disk f, and is supported and levelled by three levelling-screws n, n', n''. Through the centre of the ebonite disk passes a brass centre-pin D D′ (fig. 2), on which is centred a brass arm, H H′, capable of

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* A detailed and most careful comparison of these coils was made by Prof. G. Chrystal and Mr. S. A. Saunder in 1875; and their Report is printed *in extenso* in the Report of the British Association at Glasgow in 1876. This is the most recent occasion on which these coils have been examined.

revolving round just clear of the disk. Beneath the arm, and soldered to it, is a short brass spring *x*, which depends vertically downwards. This spring carries at its extremity a small prism of platinum-iridium with one edge vertical and turned inwards. In the groove turned in the disk *e* is stretched a platinum-iridium wire about $\frac{3}{32}$ of an inch in diameter. The wire extends round about $\frac{25}{27}$ of the circumference, and is about 39 inches long; and the groove is of such a size that the wire lies with exactly half its thickness imbedded in it. This wire is represented by the thick black line A C A′ in fig. 1. The ends of this wire are soldered to copper strips *k*, *k*. On the wood rectangles *j*, *h*, *i* is fastened an arrangement of longitudinal copper strips, *k*, *k*, which connect together eight transverse square copper bars in the manner shown in fig. 1. On the ends of these transverse bars are fixed vertical copper pins $\frac{5}{16}$ of an inch in diameter and $\frac{3}{4}$ of an inch high. On these pins are slipped short lengths of india-rubber tube which extend beyond the pins, so that they form small cups about 1 inch deep, *p′* (see fig. 3). The top of the copper pin is well amalgamated with mercury, and forms the bottom of the cup. These cups are filled about a quarter full of mercury. On the longitudinal strips of copper are fixed three binding-screws B, B′, G; and a fourth (G′) simply goes through the wood, and is connected by a wire *t* underneath the baseboard with the centre-pin D, and is therefore in metallic connexion with the spring *x*. The battery is connected with the terminals B, B′, and the galvanometer with the terminals G, G′. To the arm H H′ is adapted a trigger, T, of such shape that when the button *w*, which is of ebonite, is pressed down, the spring *x*, carrying the platinum-iridium knife-edge, is bent inwards until it touches the wire strained round the circumference of *e*. The arm carries a vernier N, which travels round sunk in a shallow groove in the face of the ebonite disk; and the ebonite is graduated on the face on the margin of the groove. The graduations are cut into the ebonite, and then rubbed over with powdered chalk mixed with gum and water. This gives a graduation very legible and pleasant to look at. The length of the wire is just one thousand divisions; and the vernier enables these to be divided into tenths. The zero of graduation is so placed that, when the pointer of the vernier reads zero, the knife-edge on the spring x is exactly opposite the extremity of the platinum-iridium wire.

It is thus clear that the revolving arm carrying its knife-edge can be moved round so that, on pressing the trigger-button w, the knife-edge makes contact at any point of this wire, and thus connects this point with the terminal G′.

This part of the arrangement answers to the sliding block and piston-contact piece of the ordinary divided-metre bridge.

3. *Method of using the Balance.*—Let now two resistance-coils of about equal resistance be provided, and let the coil-terminals of one coil be placed in the mercury-cups p and r, and those of the other be placed in q' and s'. And let two more coils be taken of not very unequal resistance which it is desired to compare with each other, let the terminals of one be placed in the mercury-cups a and c, and those of the other in b' and d'. It will then be seen that if a battery be connected with B B′, and a galvanometer with G G′, that we have the usual Wheatstone's bridge arrangements (see fig. 5 on page 179, which gives a diagram of the connexions). Two quart Leclanché cells are best suited for ordinary use. If a more powerful battery is used, there is danger of heating the platinum-iridium wire, and so expanding it that it may slip down out of its groove.

The coils in the intervals between the cups p and r and q' and s' form two branches; and the coil in the interval between a and c, together with the resistance of the platinum-iridium wire round to the place where the spring x touches it, forms the third branch, whilst the coil in the interval b' d', together with the remainder of the wire, forms the fourth. The "bridge"-wire consists of the arm H H′ and the wire under the base-board together with the galvanometer inserted between G and G′. By moving round the arm H H′ and pressing the button w, we can find a position where there is no current through the galvanometer. The copper strips k k are made of copper so thick that their resistance is practically nothing. Having established a balance between the conductors and read the vernier, the next operation is to lift up the legs of the coil which were inserted in the cups a and c and drop them into the cups b and d. Likewise a similar change is effected on the other side; the terminals of the coil inserted in b' and d' are changed to a' and c'. An examination of the connexions as shown in fig. 1 will show that the result of the operation is as if the coils had *changed places* whilst preserving their former connexion. Now let the arm be moved round and a fresh position of equilibrium found by pressing the trigger and reading the vernier. A little consideration will show that the difference of these readings gives the difference between the resistances of the coils in terms of a length of the bridge-wire; for the amount by which one coil exceeds the other in resistance is equal to the resistance of that part of the bridge-wire included between the two readings*. In order to render this method of determining the difference of the two coils practicable, the platinum-iridium wire must be exceedingly uniform in resistance, or else a Table of calibration will have to be made. Great pains were taken to procure a length of wire as uniform in size and resistance as possible; and considerable care was taken, in laying the wire in its groove, not to strain it in any way. It lies evenly in its groove, just sufficient tension being put upon it to keep it in its place. The whole resistance of the wire from end to end is not far from $\frac{1}{20}$ of an ohm at about 15° C.

The wire was carefully calibrated by measuring the difference in the resistance of two pieces of thick brass wire of such lengths that the difference of their resistances was about equal to that of thirty divisions of the bridge-wire; and this difference was measured at about a hundred different equidistant positions all along the bridge-wire, and found to be so nearly the same that no Table of calibration was deemed requisite. To protect the bridge-wire from injuries, as well as to preserve it from being heated by radiation from surrounding bodies, a wooden ring, *v v*, is fastened down on the base-board. The ring is $1\frac{1}{4}$ inch wide and $\frac{3}{4}$ inch deep; and its internal diameter is 1 inch greater than that of the ebonite disk. The wire, therefore, lies hidden away on the side of a square-sectioned circular tube; and, furthermore, a shield of cardboard faced with tinfoil lies upon the face of the disk *e*, extending just beyond the ring. An aperture is cut in this shield to permit the passage of the trigger, as well as to allow the vernier to be read. By this means the wire is not only out of sight, but out of reach of all radiation as well as mechanical injury.

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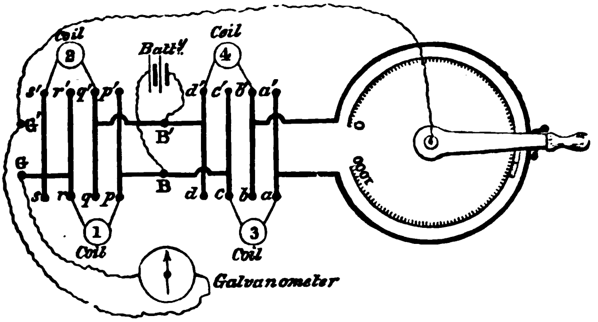
* This method of obtaining the difference of two resistances in terms of a length of the calibrated bridge wire was suggested by Prof. G. C. Foster, F.R.S., in a paper read before the Society of Telegraphic Engineers, May 8, 1872. In this paper is given an account of the method of calibrating a wire. It is obvious, without any further proof, that if the coil placed in *a* and *c* exceeds in resistance that placed in *b* and *d*, then on exchanging them, since the united resistance of coils and bridge-wire remains the same, that the contact knife-edge must be moved back along the bridge-wire by a length exactly equal in resistance to the excess of one coil over the other.

Fig. 5.



4. *Method of determining the Variation-coefficients of Coils.*—To determine the variation-coefficient of any given coil we proceed as follows:—Three other coils are provided, two of them nearly equal in resistance, which we will call 1 and 2. A third coil, 3, must be taken whose resistance is nearly equal to that of 4, the coil whose variation-coefficient is desired (see fig. 5). The terminals of 3 are inserted in the mercury-cups *a* and *c*, those of 4 in *b'* and *d'*, those of 1 in *p* and *r*, and those of 2 in *q'* and *s'*. Now the operation to be conducted is to keep the coils 1, 2, and 3 at a fixed temperature, and to keep 4 successively at two known temperatures, differing by about 15° Cent., and to obtain the difference of the resistances of 3 and 4 at these two temperatures. The difference of these differences, divided by the difference of the temperatures, is the mean coefficient of variation of resistance between these temperatures. The chief difficulty to be contended with is that of keeping the temperature of the coils constant during the operation, and of ascertaining what that temperature is; for, as Prof. Chrystal has remarked in his report (Brit. Assoc.

Report, 1876), it is not easy to tell whether the temperature of the water in which the coil rests is identically the same as that of the wire, since the latter is imbedded in a mass of slowly conducting paraffin. To reduce as far as possible the difficulty of keeping the coils at a constant temperature, they are placed in water-vessels made of zinc (see fig. 3, Pl. XIX.). These water-boxes are composed of two cylindrical vessels—an outer case 9 inches high and 8 inches in diameter, and an inner one of lesser size; the two are connected at the top, so that they form a sort of jar with hollow sides and double bottom. This interspace forms an air-jacket. Around the inside vessel near the top is a row of small holes; and two tubes communicate at the bottom—one with the inner vessel, and the other with the annular interspace. The top is closed by a wooden lid with apertures for thermometer and stirrer. Water can be made to flow from the supply-pipes into the inner vessel; it rises up and overflows through the holes, and drains away down the interspace and out by the other pipe. The bodies of the four coils are placed in four water-boxes of this description; and water from the town mains being sent in a continuous stream through all four water-boxes, the coils are rapidly brought to and maintained at a known temperature. Any desired temperature can be given to one coil by leading warm water from a cistern into its vessel. The annular air-filled space renders the rate of cooling very slow. Hence the coils, once at the desired temperature, can easily be kept there. Fig. 4, Pl. XIX., gives a sketch of the arrangement, two of the water-boxes being removed to show the connexions.

The advantage of the somewhat complicated arrangement of copper bars will now be seen. We can, without withdrawing the coils 3 and 4 from their water-boxes, and without in any way disturbing the other arrangements, *reverse* the position of the coils 3 and 4 on the bridge, by simply lifting up the legs half an inch and changing the mercury-cups into which they dip. Thus the legs of coil 3 are changed from cups a and c to b and d, and those of coil 4 from b' and d' to a' and c'. This exchange does not occupy more than a few seconds; and hence we can obtain the two readings necessary to give the difference of the resistance of the coils 3 and 4 when they are at different temperatures in a very short time. During this short time the temperatures of the two coils will not change perceptibly, protected as they are by an air-jacket.

In the ordinary form of straight bridge there is considerable trouble in exchanging the coils, because the water-vessels have to be moved and the mercury-cups readjusted; and all this time the coils are cooling; so that the two readings are never made under the same circumstances as regards temperature. Beginning, then, with all four coils at the same temperature, we take the difference between 3 and 4. To get them all at the same temperature, water from the town mains is allowed to circulate through the system for half an hour. At the end of this time the difference of 3 and 4 is taken; and several readings are taken at small intervals of time to see if the temperatures are constant. This being done, the temperature of coil 4 is raised by the introduction of warm water until it is about 15° above that of coil 3. It is best to raise the temperature about 20° above the other at first, and keep it there for 20 minutes, and then let it fall very slowly. In this way coil and water cool together, and an equilibrium of temperature is established between them. The difference between 3 and 4 is again taken; and from these two readings we have, as seen above, the mean variation-coefficient between the two temperatures. Another method, which would probably be a more accurate one, for obtaining the mean coefficient of variation between 0° C. and 15° C. would be to wait until the temperature of the water in the town mains was about 15° C., and then to keep three of the coils at that temperature, and to cool the fourth by means of ice to zero. If then all four were kept at 15° and the observations repeated, we should have the means of finding the variation-coefficient of the fourth coil between 0° and 15°. Prof. Chrystal, in his report, threw out the suggestion that resistance-coils should have a thermoelectric couple attached to them, one junction being buried in the heart of the paraffin surrounding the wire, and the other outside. This has been tried in some coils recently made, and proves a satisfactory method of ascertaining the equilibrium of temperature between the wire and the water.

Another source of error in the ordinary methods arises from uncertain or variable resistances at the mercury-cups. It is important that the copper legs of the coil-terminals should press very firmly against the tops of the copper pins on which the india-rubber-tube cups are fixed. To ensure this, the plan adopted is to fasten on the coil-legs an ebonite clamp. Along the edge of the wooden promontory, *j h i* (fig. 1), are put brass pins *m*; and by means of steel spiral springs fixed to these and attached to the clamps the coil-legs are pressed down very firmly (see fig. 3). The ends of the pins which carry the india-rubber cups and the ends of the coil-legs being well amalgamated, we get, when they are thus firmly pressed in contact, a very good joint, and one whose resistance is small and constant. If the clamps are not used, then one leg may get lifted up a little, and thus a short length of mercury interposed, which leads to an error in a reading.

5. *Example of a determination of the Variation-coefficient of a Coil.*—The whole resistance of the platinum-iridium wire is very nearly 0·0512 of an ohm, or not far from $\frac{1}{20}$ of an ohm, at about 15° Cent. As the whole length can be divided by the vernier into 10,000 parts, this gives as the value of $\frac{1}{10}$ of a division $\frac{1}{2000000}$ of an ohm.

The unit in the following example is $\frac{1}{10}$ of a division. To secure the greatest accuracy of measurements a low-resistance galvanometer must be used. I am in the habit of using one having a resistance of about half an ohm. The image of a wire strained across a slit is reflected on a scale in the usual way, and read at a distance by means of a telescope. This galvanometer will give an indication, when used with precautions, due to a difference of one tenth of a division when comparing two ohm coils. But as the temperature can hardly be measured with certainty to within less than $\frac{1}{20}$ of a degree, this alone renders such refinement of reading nugatory, in the absence of better methods of ascertaining with certainty the real temperature of the wire.

Two coils were compared. Call them F and K. K is the coil whose variation-coefficient is required.

I. *Difference of Resistance of Coils* F *and* K *at* 11° *Cent.*

	Bridge-readings.		Difference.
Exp i.	5000	4955	45
Exp. ii.	5000	4954	46
Exp. iii.......	5000	4955	45

The first column gives the number of experiment, the second the reading with the coils F and K in one position on the bridge, the third when F and K are reversed or have exchanged places on the balance; and the fourth gives the difference of their resistances at 11° C. in units of the bridge-wire.

II. *Difference of Resistance of Coils* F *and* K *at* 28°·2 *Cent.*

	Bridge-readings.		Difference.
Exp. i.	5439	4492	947
Exp. ii.	5442	4497	945
Exp. iii.......	5440	4490	950

As before, the fourth column gives the difference of F and K at 28°·2 C. Taking the mean difference at 28°·2 C. to be 947 units, and that at 11° C. to be 45 units, we have

$$\frac{947-45}{28{\cdot}2-11}=52{\cdot}4 \text{ units}$$

as the mean variation-coefficients between 11° C. and 28° C. in units of bridge-wire. Since the coils F and K are approximately ohm coils, this gives as the variation-coefficient of the coil K ·0262 per cent. This coil is of platinum-silver wire. These three determinations occupied about an hour and a half, during which time many more readings were taken, all closely agreeing with the above. The actual measurement of the differences requires but a few moments to effect, the principal expenditure of time being that required to bring the coils to the same temperature as the water.

In conclusion, I may state that this resistance-balance has been constructed in the workshops of the School of Mechanical Engineering at Cambridge, under the direction of Prof. Stuart. Great care was taken in laying on the wire so as to avoid straining it in any way; and the performance of the instrument is consequently very satisfactory. I should also express the fact that this excellent performance is due to the supervising care of Prof. Stuart, who not only supplied several of the details of the construction, but aided, by his valuable suggestions generally, during the process of carrying out my rough designs into a practical form.

Cavendish Laboratory, Cambridge,
December 1879.

XXVII. *A Dispersion-Photometer.*

By JOHN PERRY *and* W. E. AYRTON[*].

IN measuring what is usually termed the power of a light, it is common to have a screen placed at such a distance from the light that its illumination is equal to that which it or another screen receives from a standard candle. Now, if a standard candle is, say, one foot from a screen, an electric light of, say, 6400 candle-power must be placed at the distance of 80 feet from a screen to give the same illumination. That a great distance like this should be necessary, and in a chamber whose walls are supposed to be blackened, in the laboratories of works where electric lights are usually examined, has placed great difficulties in the way of exact determinations of the powers of lights. Our experience in experimenting with electric lights leads us to believe that, but for this difficulty, we should have a vast body of information on the subject of electric lighting, instead of the vague and conflicting statements which fill the scientific journals. Perhaps only those who have made experiments will appreciate fully the great advantages of our having some compact form of photometer. The members of the Society will no doubt see many applications of a compact photometer, such as measuring the light from various parts of the sky for instance, in which ordinary photometric methods are unavailable. Now our instrument will resemble a camera; and it may be turned in all directions.

We were delighted to hear from Dr. Guthrie that his principle of a "retention-image photometer," recently communicated to the Society, has proved to be correct quantitatively. The success of his test must be as interesting to physiologists as to physicists. Besides testing his principle by using powerful lights, he will have experiments to make concerning the loss of light in reflection, as we shall have concerning loss in refraction. Probably, however, he does not intend to apply his instrument to powerful lights, on account of the very great difficulty he would meet with in measuring the breadth of the fine slot which would be needed.

In ordinary photometric methods, the rays of light illuminating unit area of screen, if coming from a powerful source, are contained in a very small solid angle. We use the very simple expedient of causing these rays to fill a much greater solid angle by passing them through a thin concave lens, and in this way obtain the same amount of illumination as before, but on a screen placed at a short distance behind the lens. Thus we not only save space, but prevent a great deal of the absorption which occurs when light passes through air. This absorption is sometimes very great in London. We have, however, absorption in the lens as a disadvantage; but with a very thin lens this may probably be reduced to an almost inappreciable amount.

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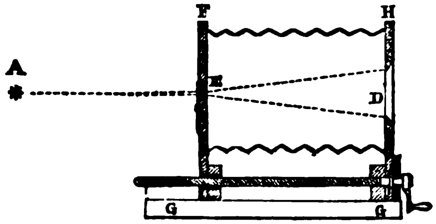
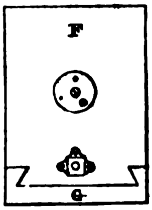
[*] Read December 13th, 1879.

A is the light to be measured ; D is a paper screen illuminated by light passing through the concave lens E. The frame F, which carries the lens, slides on a stand G, which has marked divisions, so that a pointer tells the distance from the focus of the lens to the screen D. The sides of the space F D are of black cloth, like the sides of a folding camera; and the inside is all blackened except the screen D. A circular plate with three round holes of different sizes is in front of the lens. A diaphragm of this kind is not necessary, if we can be assured of there being no reflection from the inside of the box. Beside the instrument, and containing the standard candle, is a box blackened inside, in one end of which is a screen similar to D. If D is the distance from the light to the lens and d is the distance from the principal focus of the lens to the screen; if δ is the focal length of the lens, then, roughly, a bundle of rays of unit solid angle gets to have, after refraction, an angle





$$\frac{D^2}{\delta^2},$$

as D is always great in comparison with δ. If L is the total light, then the unit angle of incident rays and the unit angle of refracted rays have amounts of light

$$\frac{L}{4\pi}\text{ and }\frac{L}{4\pi}\cdot\frac{\delta^2}{D^2};$$

so that a screen at the distance d from the focus of the lens has illumination of the intensity

$$\frac{L}{4\pi}\cdot\frac{\delta^2}{D^2}\cdot\frac{1}{d^2}.$$

If, now, another screen has the same illumination from a candle whose total light is unity, at the distance D_1 this illumination is

$$\frac{1}{4\pi D_1^2}=\frac{L}{4\pi}\cdot\frac{\delta^2}{D^2}\cdot\frac{1}{d^2};$$

and hence

$$L=\frac{D^2}{D_1^2}\cdot\frac{d^2}{\delta^2}.$$

A double-concave lens, of focal length 1 centimetre, being employed, and d being capable of variation from 40 to 10 centimetres, and D being as much as five times D_1, we can measure in a very small space a light which is from 40,000 to 100 times the standard candle.

We propose to make careful experiments on the absorption in passing through the lens; and by interposing thin plates of glass between the standard candle and its screen to produce there a similar absorption, we have no doubt that measurements may be made with this instrument with much greater accuracy than is possible by the ordinary method.

The power of the light is practically, therefore, proportional to the square of the product of the measurable distances. It will be, of course, unnecessary to measure the variations both in D and in d, as the scale can be so graduated as to give both distances at one reading.

XXVIII. *On Intermittent Currents and the Theory of the Induction-balance.* *By* OLIVER J. LODGE, *D.Sc.*[*]

1. THE telephone, considered as a scientific instrument, seems destined to play an important part as a detector of minute currents of rapidly changing intensity; and the general theory of intermittent currents is being brought into prominence by its use.

The equations to which most attention has been hitherto directed have been those relating to the steady flow of a current after the initial inductive or inertia-like effects have subsided; and in arrangements such as the Wheatstone bridge, a double key is commonly used, in order to allow the introductory stage to subside before any observation is taken. The galvanometer is essentially an instrument for measuring steady currents, or for giving the algebraically integrated expression for the total quantity of electricity which has passed in the case of transient currents; that is, $\int_0^\tau i\,dt$, τ being small compared with the period of swing of the galvanometer-needle.

Again, the electrodynamometer has an important use as an integrator of the current without paying attention to sign; that is, its indications give the value $\frac{1}{\tau}\int_0^\tau i^2\,dt$, where again τ, the total duration of the current, is small.

But the telephone-plate has such a very small period of swing that the same τ, which is vanishing compared with the time of oscillation of a needle, may be many times greater than that of the telephone-plate. Moreover the plate is not limited to one mode of vibration, but can have minor vibrations superposed on the fundamental; so that it can enter into the changes going on, and render minute fluctuations audibly apparent which would in more slowly moving detectors be simply merged in the total effect.

Thus a rapidly alternating current (such as the telephone itself produces), which is totally unfelt by a galvanometer, is appreciated by a telephone-plate in its variability—the pitch of the note indicating the number of vibrations, even if they are so rapid as to produce a shrill whistle. The telephone, in fact, does not integrate the current, but gives all its fluctuations within certain limits.

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[*] Read on the 24th of January, 1880.

The complete theory of the telephone, setting forth precisely on what the loudness of its indications depends, would be a most interesting and important investigation; but if it has been attacked, I am ignorant of it. It seems probable that the loudness of the sounds will be found to depend upon the amplitude of the vibrations of the plate and upon their velocity conjointly—in other words, both on the total change of the current and on the rate at which the change takes place; *i. e.*, in the symbols hereafter to be used, that the loudness is a direct function of $j\frac{dj}{dt}$. I shall not, however, assume anything of this sort, but shall content myself with simply finding the value of the current j as a function of the time, leaving the rest to be done subsequently.

It is quite true that the telephone is only an indicator and not a measuring-instrument; but so many null methods can be devised which permit measurements to be made with a simple detector, that it is probable that it will have important applications in this capacity also. And hence I think the general theory of intermittent currents, or of currents in the variable stage, will come into more prominence than hitherto.

The induction-balance furnishes an illustration. Dove made experiments with it; and Felici established the laws of current induction by its aid: but its power as an instrument of research was never appreciated till Prof. Hughes applied to it an intermittent current and a telephone.

Faraday interposed blocks of copper between a primary coil and a secondary connected with a galvanometer, and was surprised to find that the effect at make and break was precisely the same with the copper as without it[*]. It was, however, afterwards found that the physiological effects were very different, being much less when the copper was present—thus proving that though the copper did not affect the integral flow of electricity, yet it greatly affected the time during which that flow took place.

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[*] Exp. Res. arts. 1721, 1735.

Dove repeated Faraday's experiment more perfectly by means of an induction-balance, and showed that no non-magnetic media produced any effect appreciable by a galvanometer.

Thus may numerous phenomena be overlooked with a galvanometer which with a telephone become obtrusively evident.

2. My attention was more particularly directed to the subject by an observation which was made by Mr. W. Grant, assistant in the Physical Laboratory, University College, and which I have his permission to describe.

The intermittent current from a mouth-telephone, or the induced current from a clock-ticking microphone and coil, is sent through a long helix of wire wound upon a bobbin, with another similar but quite disconnected wire wound alongside it. A telephone and ear are also arranged in the circuit of the first wire, and the loudness of the sound observed. The disconnected wire, which is wound on the same reel as the first, now has its two ends joined up so that it itself forms a closed circuit: the loudness of the sound is thereby considerably increased. The secondary circuit is broken again, and the sound again becomes faint. The strengthening on closing the secondary circuit is so great, indeed, that short-circuiting the primary so as to shunt out all its resistance, scarcely produces any additional strengthening effect. In other words, a moderate resistance (several ohms) may be thrown into a telephonic circuit without the slightest appreciable weakening of the effect, provided a second wire coiled alongside the first be arranged so as to form a closed circuit. If a second telephone be put in circuit with this second wire, it will give about the same indications as the other; and this may sometimes be a good way of working two telephones.

The rough general explanation is, of course, not far to seek. The extra currents, which at first oppose the primary, are able to form in the secondary when its circuit is closed; and hence the changes in strength of the primary are more rapid, and therefore more complete.

An obvious modification is, to arrange both speaking and hearing telephone in circuit with both the parallel coiled wires in series, first, so that the currents flow the same way in both wires, and then with one wire reversed; so that in the first case the self-induction is a maximum, and in the other almost zero.

Any one who tries this experiment will at once appreciate the very decided difference in the telephonic indications. In fact, resistance is of much less consequence in a telephone-circuit than is self-induction. This fact obviously explains the discrepancy which has been observed between experiments on resistance-bobbins thrown into a telephonic circuit in a laboratory, and the same resistance when forming an actual line. The resistance-coils are wound so as to have no self-induction; and they accordingly weaken the effect very little. It is surprising how many thousand ohms can be thrown into a talking circuit without stopping a conversation. But if the bobbins were wound all in one direction, they would be still worse than a line-resistance.

3. It is plain from this that the only right method of arranging a telephone-line is to have two wires running close together—one for the direct, the other for the return current. This arrangement has, I believe, been used in order to avoid induction-effects from battery-currents in neighbouring wires; but it has also the great advantage of diminishing the self-induction of the wire itself. It is, however, a question how far the increase in electrostatic capacity would militate against this advantage in a very long land-line. It is certain to be injurious as far as it goes; but if the wires are very thin, their electrostatic capacity will be but small.

In the case of a cable the double wire would have decidedly less capacity than a single wire; and the capacity diminishes rapidly with the size of the wire. Hence it appears that a telephone-cable should have a pair of exceedingly fine insulated wires of the very highest conductivity pretty close together in a large insulating sheath.

I need hardly say, perhaps, that no advantage will be gained by attempting to do away with the self-induction of the telephone-bobbin itself by means of a second wire wound alongside the first and forming a closed circuit—because the intermittent currents induced in this would be nearly as strong as those in the direct circuit, and, being always in an opposite direction, will nearly neutralize their intermittent effect on external coils or on the telephone-plate.

I propose now to work out the theory of the induction-balance, and of one or two other cases of intermittent currents, as completely as possible without taking into account the electrostatic capacity of the wires. It would be very interesting to obtain the perfectly general equations; but they are very complex, and appear to be beyond me at present.

General Theory of Current-Induction.

4. The general theory of the establishment of a current in a number of circuits of known resistance, in one or all of which a given electromotive force begins to act, was begun by Helmholtz and Thomson, who independently showed that the fact of the existence of induced currents, discovered experimentally by Faraday, as well as the laws of the same, could be deduced mathematically from the electromagnetic attractions of Oersted and Ampère by means of the principle of the conservation of energy. The complete theory, it is needless to say, is to be found in Maxwell's 'Electricity,' and also in that admirable digest of the whole subject, Professor Chrystal's article in the *Encyclopædia Britannica.*

Suppose there are n circuits of resistances $r_1, r_2, \ldots,$ in each of which "impressed" electromotive forces $E_1, E_2 \ldots$ begin to act at the same instant; then the strengths of the currents $i_1, i_2 \ldots$ in the several circuits at any time t afterwards are to be found by the following set of n equations—

$$\left.\begin{aligned}
E_1 - r_1 i_1 &= \frac{d}{dt}(M_{11} i_1 + M_{12} i_2 + \ldots + M_{1n} i_n),\\
E_2 - r_2 i_2 &= \frac{d}{dt}(M_{21} i_1 + M_{22} i_2 + \ldots + M_{2n} i_n),\\
\cdot \quad \cdot \quad \cdot \quad &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot\\
E_n - r_n i_n &= \frac{d}{dt}(M_{n1} i_1 + M_{n2} i_2 + \ldots + M_{nn} i_n),
\end{aligned}\right\} \quad . \; . \quad (1)$$

where M_{lk} is a double integral, called the coefficient of mutual induction between the circuit l and the circuit k.

Induction-balance.

5. Now in the induction-balance as ordinarily arranged there are three such circuits, with the coefficients M all constant, and two of them without any electromotive force. The first, or battery-circuit, consists of a battery and two equal

coils, and it also contains some kind of intermittent break or microphone; the second is the telephone-circuit, and consists of two equal coils and a telephone; and the third is complete within the coin or other piece of conducting matter in the neighbourhood of one pair of the coils of the other circuits. The general value of the telephonic current at any instant after a make has occurred in the primary would therefore be obtained by the solution of a linear differential equation of the third order with constant coefficients.

But the problem may be simplified by observing that the "coin" is not introduced until a perfect "balance" has been obtained between the first and second circuits—that is, until they are conjugate to one another so that $M_{12}=0$. Moreover the coefficients of induction between the two coiled circuits and the coin are ordinarily sufficiently small for their squares to be neglected; in which case, as will be shown more fully later (sections 12 and 13), the above set of equations reduces to the following—

$$\begin{aligned}
M_{11}\frac{di_1}{dt}+\quad 0\quad+\quad 0\quad+r_1i_1&=E,\\
0\quad+M_{22}\frac{di_2}{dt}+M_{23}\frac{di_3}{dt}+r_2i_2&=0,\\
M_{31}\frac{di_1}{dt}+\quad 0\quad+M_{33}\frac{di_3}{dt}+r_3i_3&=0,
\end{aligned}$$

or, as I shall for greater convenience write them,

$$\left.\begin{aligned}
L\frac{di}{dt}+Ri&=E,\\
l\frac{dj}{dt}+m\frac{dk}{dt}+rj&=0,\\
\lambda\frac{dk}{dt}+\mu\frac{di}{dt}+pk&=0,
\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (2)$$

where the three coefficients of self-induction are written L, l, and λ (λ being very small); and where the battery-current is i the telephone-current is j and is small, and the current in the coin is k; m signifies the effect of the coin-current on the telephone-circuit, and μ the effect of the primary circuit on the coin. The effect of the primary on the secondary (M) is, as already explained, supposed to be 0; that is, balance is supposed to have been obtained before the insertion of the coin. ρ, or the resistance which the current circulating in the coin experiences, is a quantity which is likely to be several times larger than the specific resistance of the material (*i. e.* the resistance of a centimetre cube); but for ordinary coins it is a number of the same order of magnitude. It must be remembered that R and L stand for the resistance and self-induction of the *whole* of the battery-circuit, and, similarly, r and l include the telephone-coil as well as the other two coils of the telephone-circuit.

Now the only difficulty in solving these equations consists in the varying resistance of the battery-circuit, in which a contact-breaker, or clock-ticking microphone, or some other arrangement for producing an intermittent current is inserted. I shall therefore leave the consideration of the induction-balance for the present, and examine the case of a primary circuit in space by itself and having a break and make in some part of it. Then we will consider the case of an intermittent primary in the neighbourhood of a closed secondary; and after this it will be easy to apply our results to the induction-balance.

On the Law of Variation of a Battery-current in a solitary Circuit.

6. Let a battery of constant electromotive force E act in a circuit whose total resistance may be *suddenly* changed from the value R to the value S, and let L be the coefficient of self-induction of this circuit. Then the current i at any time t after the change of resistance has occurred is to be found from the equation

$$\mathrm{L}\frac{di}{dt}+\mathrm{S}i=\mathrm{E}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

with the initial condition $i=\frac{\mathrm{E}}{\mathrm{R}}$ when $t=0$. This gives us in the integral form

$$i=\frac{\mathrm{E}}{\mathrm{S}}\left(1+\frac{\mathrm{S}-\mathrm{R}}{\mathrm{R}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}\right). \quad . \quad . \quad . \quad . \quad (4)$$

Putting $\mathrm{R}=\infty$, we get the well-known expression for the current at "make,"

$$i=\frac{\mathrm{E}}{\mathrm{S}}(1-e^{-\frac{\mathrm{S}}{\mathrm{L}}t}). \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Putting S very large, we get an expression for the current at partial break, provided there is no extra-current spark at the surface of separation,

$$i=\frac{\mathrm{E}}{\mathrm{R}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It will not do to put $\mathrm{S}=\infty$, because it is impossible to make the resistance *suddenly* infinite, *i. e.* to stop the current instantaneously.

7. It is difficult to get a reasonably correct expression for the value of the current at break. We may suppose that, by separation of two portions of the circuit, the resistance is suddenly changed from R to a quantity which would be S if the current instantaneously ceased, but which is much less than S for a very short time, owing to the heat generated by the current itself. The temperature of the spark at any time t after the break is to be found, we may suppose, from such an equation as this,

$$mc\theta=\int_0^t (ri^2-\mathrm{H}\theta)dt,$$

where H is the cooling-constant. The mode in which the resistance of air changes with temperature is unknown; so we may assume any simple and not improbable law, such as that the decrement of resistance is proportional to the increment of temperature, or

$$dr=-k\,d\theta,$$

where k is a constant. From these two equations r has to be obtained as a function of the current i and of the time t; and then its value has to be substituted for S in equation (3). As a first approximation, we may imagine the time too short for cooling, or make $\mathrm{H}=0$. Then we get

$$r=\mathrm{S}e^{-k'\int_0^t i^2dt};$$

an expression which, as a coefficient in equation (3), to me seems quite unmanageable. I will therefore assume for the future that the change in the battery-circuit resistance is always made suddenly from one finite value to another, no spark or current across air-spaces being produced.

8. *Suppose the resistance of a battery-circuit oscillates rapidly between the values* R *and* S, *each change being made suddenly*

and lasting for the short time τ; *what is the strength of the current after a few seconds?*

This may be assumed to be the sort of thing that happens in a microphone or other arrangement for producing an intermittent current.

The strength of the current before the vibration begins is

$$i_0 = \frac{\mathrm{E}}{\mathrm{R}};$$

at the end of the first period τ the strength is, by equation (4),

$$i_1 = \frac{\mathrm{E}}{\mathrm{S}} + \frac{\mathrm{E(S-R)}}{\mathrm{SR}} e^{-\frac{\mathrm{S}}{\mathrm{L}}t};$$

at the end of the second period τ the current is

$$i_2 = \frac{\mathrm{E}}{\mathrm{R}} - \frac{\mathrm{E(S-R)}}{\mathrm{SR}} \left(e^{-\frac{\mathrm{R}}{\mathrm{L}}\tau} - e^{-\frac{\mathrm{R+S}}{\mathrm{L}}\tau}\right),$$

and so on—

$$i_4 \text{ being } \frac{\mathrm{E}}{\mathrm{R}} - \frac{\mathrm{E(S-R)}}{\mathrm{SR}} (\text{exp.}) \{(\mathrm{R}) - (\mathrm{R+S}) + (2\mathrm{R+S}) - (2\mathrm{R}+2\mathrm{S})\} \left(-\frac{\tau}{\mathrm{L}}\right)$$

and

$$i_5 \text{ being } \frac{\mathrm{E}}{\mathrm{S}} + \frac{\mathrm{E(S-R)}}{\mathrm{SR}} (\text{exp.}) \{(\mathrm{S}) - (\mathrm{S+R}) + (2\mathrm{S+R}) - (2\mathrm{S}+2\mathrm{R}) + (3\mathrm{S}+2\mathrm{R})\} \left(-\frac{\tau}{\mathrm{L}}\right)$$

Accordingly, after a very large number of vibrations (which will be accomplished in a few seconds) the current is either

$$i = \frac{\mathrm{E}}{\mathrm{S}} + \frac{\mathrm{E(S-R)}}{\mathrm{SR}} \left(\frac{1 - e^{-\frac{\mathrm{R}}{\mathrm{L}}\tau}}{1 - e^{-\frac{\mathrm{S+R}}{\mathrm{L}}\tau}}\right) e^{-\frac{\mathrm{S}}{\mathrm{L}}t}, \quad . \quad . \quad (7)$$

or else the same expression with R and S everywhere interchanged, according as the last change of resistance was from R to S or from S to R, t being the time which has elapsed since the last change.

The law of variation of such a current is therefore just the same as for a simple make and break (4); but the part depending on time is multiplied by a constant fraction always less than unity, and which diminishes rapidly as τ (the period of a semi-vibration of the intermittence) diminishes.

If τ be infinitesimal compared with $\frac{L}{R+S}$, the fraction is $\frac{R}{S+R}$, and hence the current is

$$i=\frac{E}{S}\left(1+\frac{S-R}{S+R}e^{-\frac{S}{L}t}\right),$$

or, as of course t (being less than τ) is itself vanishing,

$$i=\frac{2E}{S+R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Hence the current tends to approach this constant value if the vibrations are too rapid. Such a current as this is of course inappreciable by a telephone; but the above would be its galvanometric indication.

This must be something like the state of things in a coil with a too rapid break.

Query, whether any thing of the same sort happens when a battery-current is passed through a vacuum-tube, the intermittence being almost too rapid to be heard by the telephone. Drs. De la Rue and Hugo Müller have put a telephone in circuit with a battery and vacuum-tube, and heard only a faint rustling when the stratifications were steady.

Equation to a continuously Intermittent Current.

9. It would be interesting to obtain an expression for the current when the resistance of the circuit varies *continuously* from some finite value to infinity and back again in a short regular period $\frac{\pi}{\omega}$. Thus R might be supposed to be $\rho\sec^2\omega t$; and the equation would be

$$L\frac{di}{dt}+\rho i\sec^2\omega t=E, \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

of which the solution is

$$i=Ce^{-\frac{\rho}{L}\tan\omega t}+\frac{E}{L}e^{+\frac{\rho}{L}\tan\omega t}\int e^{-\frac{\rho}{L}\tan\omega t}dt; \quad . \quad . \quad (10)$$

but the integral $\int e^{\tan x}dx$ does not appear to be evaluable except by an unmanageable lot of series.

Equation to an Alternating Current.

10. Instead of varying the resistance, we may produce an alternating current by a periodic electromotive force, as, for

instance, when the circuit contains an electromagnetic machine of constant resistance, such as a telephone itself. The electromotive force is then representable by a simple harmonic function or by a sum of a number of these. Take the simplest case,

$$\mathrm{L}\frac{di}{dt}+\mathrm{R}i=\mathrm{E}\sin 2\pi nt, \quad . \quad . \quad . \quad . \quad (11)$$

where the electromotive force oscillates from E to $-$E and back again n times a second. The solution of this is

$$i=\mathrm{C}e^{-\frac{\mathrm{R}}{\mathrm{L}}t}+\frac{\frac{\mathrm{R}}{\mathrm{L}}\sin 2\pi nt-2\pi n\cos 2\pi nt}{\frac{\mathrm{R}^2}{\mathrm{L}^2}+4\pi^2n^2}\cdot\frac{\mathrm{E}}{\mathrm{L}}. \quad . \quad (12)$$

The first term rapidly dies out; and so the permanent value is

$$i=\frac{\mathrm{E}}{\mathrm{R}}\cdot\frac{\sin\omega t-\lambda\omega\cos\omega t}{1+\lambda^2\omega^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

writing ω for $2\pi n$ and λ for the time-constant $\frac{\mathrm{L}}{\mathrm{R}}$.

On the Law of Variation of a Battery-current in a Polarizable Circuit.

11. In the previous sections we have considered the electromotive force E of the battery to be constant. Now this is never accurately true, as the electromotive force begins to fall off the instant the circuit is closed, and some discrepancies between theory and experiment may arise from this cause. Let us see what happens when the battery is not constant, or when a polarizable voltameter forms part of the circuit. According to Kohlrausch, the electromotive force of polarization is proportional to the amount of decomposition, and therefore a short time t after the current has been established it is

$$p\int_0^t idt,$$

where p is the electromotive force produced by the passage of a unit of electricity. The equation to the current is therefore

$$\mathrm{L}\frac{di}{dt}+\mathrm{R}i=\mathrm{E}-p\int_0^t idt,$$

or

$$\mathrm{L}\frac{d^2i}{dt^2}+\mathrm{R}\frac{di}{dt}+pi=0\,; \quad . \;\; . \;\; . \;\; . \;\; . \;\; . \quad (14)$$

and the integral of this, remembering that $i=0$ when $t=0$, is

$$i=\frac{2\mathrm{E}}{\mathrm{R}}e^{-\alpha t}\sinh\beta t, \quad . \;\; . \;\; . \;\; . \;\; . \;\; . \quad (15)$$

where

$$\alpha=\frac{\mathrm{R}}{2\mathrm{L}} \text{ and } \beta=\sqrt{\left(\frac{\mathrm{R}^2}{4\mathrm{L}^2}-\frac{p}{\mathrm{L}}\right)}.$$

If $p=\frac{\mathrm{R}^2}{4\mathrm{L}}$, there could never be any current; and it is impossible for p to be greater than $\frac{\mathrm{R}^2}{4\mathrm{L}}$. Practically, however, p would always be very small compared with this quantity; and so we may write

$$\beta \triangleq \frac{\mathrm{R}}{2\mathrm{L}}-\frac{p}{\mathrm{R}}.$$

Hence in any ordinary polarizable circuit the strength of the current at any time after "make" is

$$i=\frac{\mathrm{E}}{\mathrm{R}}\left(e^{-\frac{p}{\mathrm{R}}t}-e^{\left(\frac{p}{\mathrm{R}}-\frac{\mathrm{R}}{\mathrm{L}}\right)t}\right). \quad . \;\; . \;\; . \;\; . \;\; . \quad (16)$$

On the Law of Variation of a Battery-current when a closed secondary circuit is stationary in its neighbourhood.

12. So far we have considered a primary circuit in space by itself; but now we will arrange near it a secondary coil with a resistance r and a coefficient of self-induction l, and we will suppose M to be the coefficient of mutual induction between the primary and secondary. The coils shall be fixed in position so that M is constant; then a current j will be developed in the secondary circuit whenever the primary changes from R to S; and this will react on the primary current i, the equations for determining both being

$$\mathrm{L}\frac{di}{dt}+\mathrm{M}\frac{dj}{dt}+\mathrm{S}i=\mathrm{E} \quad . \;\; . \;\; . \;\; . \;\; . \quad (17)$$

and

$$l\frac{dj}{dt}+\mathrm{M}\frac{di}{dt}+rj=0, \quad . \;\; . \;\; . \;\; . \;\; . \quad (18)$$

with the conditions $i=\frac{\mathrm{E}}{\mathrm{R}}$ and $j=0$ when $t=0$.

Differentiating these (E being supposed constant), we easily separate the variables and obtain

$$(Ll-M^2)\frac{d^2i}{dt^2}+(Lr+Sl)\frac{di}{dt}+Sri=E, \quad . \quad . \quad (19)$$

and

$$(Ll-M^2)\frac{d^2j}{dt^2}+(Lr+Sl)\frac{dj}{dt}+Srj=0. \quad . \quad . \quad (20)$$

Now the solution of (20), remembering the initial condition, is

$$j=Je^{-\alpha t}\sinh\beta t \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

or

$$=\tfrac{1}{2}J\{e^{-(\alpha-\beta)t}-e^{-(\alpha+\beta)t}\},$$

where

$$\alpha=\frac{Lr+lS}{2(Ll-M^2)} \text{ and } \beta=\frac{\sqrt{\{(Lr-lS)^2+4rSM^2\}}}{2(Ll-M^2)} \quad . \quad (22)$$

$$\left[\text{i. e. } \alpha^2-\beta^2=\frac{rS}{Ll-M^2}\right],$$

and where J is a constant which has yet to be determined. It is well to notice that neither α nor β can vanish or become imaginary; but they can become infinite simultaneously.

To obtain the value of the primary current i, we can either write down the solution of (19), viz.

$$i=\frac{E}{S}+e^{-\alpha t}(C_1e^{\beta t}+C_2e^{-\beta t}); \quad . \quad . \quad . \quad . \quad (23)$$

or, what is practically more convenient as determining all arbitrary constants directly, we can combine equations (17) and (18) so as to eliminate $\frac{di}{dt}$, and then put in the values of j and $\frac{dj}{dt}$ from (21). We thus get

$$M(Si-E)=\tfrac{1}{2}Je^{-\alpha t}\{\sqrt{(Lr-Sl)^2+4rSM^2}\,.\cosh\beta t$$
$$+(Lr-lS)\sinh\beta t\},$$

which will give us the value of J, since $i=\frac{E}{R}$ when $t=0$. We find then that

$$J=\frac{2ME(S-R)}{R\sqrt{\{(Lr-lS)^2+4rSM^2\}}}; \quad . \quad . \quad . \quad . \quad (24)$$

and this is the value to be substituted in equation (21).

Hence, finally, we can write out explicitly the values of i and j, though, as the constants are very long, we will make an abbreviation by writing

$$\frac{4rSM^2}{(Lr-lS)^2}=\lambda^2; \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

then the strength of the battery-current at any time after the resistance of its circuit has suddenly jumped from R to S is

$$i=\frac{E}{S}+\frac{E(S-R)}{SR}e^{-\alpha t}\left\{\cosh\beta t+\frac{\sinh\beta t}{\sqrt{(1+\lambda^2)}}\right\}; \quad . \quad (26)$$

and the strength of the induced current at the same instant is

$$j=\frac{2ME(S-R)}{R(Lr-lS)\sqrt{(1+\lambda^2)}}e^{-\alpha t}\sinh\beta t. \quad . \quad . \quad . \quad (27)$$

We may notice that the expression for i contains only the *square* of M; that is, the square of M expresses the reaction of the secondary on the primary; hence when M is small this may be neglected.

Special Cases.

13. It will be interesting now to consider the special cases in which these equations (26) and (27) may be expected to assume simple forms.

Case 1. When the coils are so far apart that M^2 is negligible compared with Ll.

In this case $\lambda=0$, $\alpha+\beta=\frac{r}{l}$, $\alpha-\beta=\frac{S}{L}$; and it will be found that the expression for the primary current i as given in equation (26) reduces to the expression (4), which we found for the current in a solitary primary circuit, as it evidently ought to do; and there is nothing new to be said about it.

But the value of j, the current induced in the secondary circuit, becomes

$$j=\frac{ME(S-R)}{R(Lr-lS)}\left(e^{-\frac{S}{L}t}-e^{-\frac{r}{l}t}\right). \quad . \quad . \quad . \quad (28)$$

To find the induced current at instantaneous break in the special case now being considered, we must put $S=\infty$, and we get

$$j=\frac{ME}{lR}e^{-\frac{r}{l}t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

which is the recognized value for it (see Chrystal, 'Encyc. Brit.' equation (41)). Its initial or maximum value is $\frac{M}{l}\cdot\frac{E}{R}$; and it rapidly dies away.

To find the induced current at "make" we must put $R=\infty$; and then (28) becomes

$$j=-\frac{ME}{(Lr-lS)}(e^{-\frac{S}{L}t}-e^{-\frac{r}{l}t}). \quad . \quad . \quad . \quad (30)$$

This, therefore, begins at zero, rises to a maximum after the lapse of time

$$t=\frac{Ll}{Lr-lS}\log\frac{Lr}{lS},$$

and then dies away.

If the primary and secondary circuits are *similar*, so that

$$\frac{S}{L}=\frac{r}{l},$$

the expression for the induced current at make simplifies, becoming

$$j=-\frac{ME}{Ll}te^{-\frac{r}{l}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

14. *Case* 2.—When the primary and secondary circuits are wound side by side, so that the coefficient of mutual induction nearly equals the coefficient of self-induction of either—in other words, so that $L=l=M$.

For this case (see equations 22 and 25),

$$\lambda=\frac{4rS}{(r-S)^2}, \quad \text{and} \quad \alpha=\beta=\infty;$$

but if we put $L-M=$ a small quantity z, say, and then proceed to the limit, we shall find a finite value for the difference of α and β, viz.

$$\alpha-\beta=\frac{rS}{L(r+S)},$$

while

$$\alpha+\beta=\frac{r+S}{2z};$$

and accordingly we get as the value of the primary current (26),

$$i=\frac{E}{S}\left\{1+\frac{S-R}{R(r+S)}\{re^{-\frac{rS}{L(r+S)}t}+Se^{-\frac{r+S}{2z}t}\}\right\}. \quad (32)$$

The last term in these brackets decreases at a nearly infinite rate; hence the primary current jumps almost suddenly from the value $\frac{E}{R}$ to the value

$$\frac{E}{R} \cdot \frac{r+R}{r+S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

and then increases (or decreases, as the case may be) at a more moderate pace, its subsequent values being given by the equation

$$i = \frac{E}{S}\left(1 + \frac{r(S-R)}{R(r+S)} e^{-\frac{rS}{L(r+S)}t}\right). \quad . \quad . \quad . \quad (34)$$

The form of expression (32) when S is made very great is noticeable, as it shows that the primary current in a wire coiled up with a closed secondary is able to stop nearly as dead as if the primary were doubled upon itself (6). It is

$$i = \frac{E}{R} e^{-\frac{S}{2l}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

The value of the induced current under the same circumstances (viz. when the two circuits are coiled close together) is easily obtained from (27), and is

$$j = \frac{E(S-R)}{R(r+S)} e^{-\frac{rS}{l(r+S)}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

If $S = \infty$, this is the current at instantaneous break, viz.

$$j = \frac{E}{R} e^{-\frac{r}{l}t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

the same as (29) would have given; but if $R = \infty$, it is the current at make, viz.

$$j = -\frac{E}{r+S} e^{-\frac{S}{r+S} \cdot \frac{r}{l}t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

The induced current at *make* in this case therefore has an instantaneous maximum value $\frac{E}{r+S}$ and it then dies away at about half the rate of the current at break.

All this is very instructive. It must be the state of things approximated to in many arrangements.

15. We can now see what happens in Mr. Grant's case of a primary wound alongside a secondary, whose circuit can be

closed or unclosed; though the state of things considered in the last section is only roughly attained, because the telephone-coil forms part of the battery-circuit, and adds materially to the self-induction of that circuit without contributing to the mutual induction. However, neglecting this and taking the circuits similar, we find the primary current at "make" ($R=\infty$, $S=r$), by equation (32), to be

$$\frac{E}{S}\left(1-\tfrac{1}{2}e^{-\frac{S}{2L}t}-\tfrac{1}{2}e^{-\frac{S}{\varepsilon}t}\right),$$

whereas if the secondary circuit had been unclosed it would have been (5)

$$\frac{E}{S}\left(1-e^{-\frac{S}{L}t}\right).$$

Similarly, at the break ($S=$ large, $R=r$), the primary current, when the secondary is closed, is

$$\frac{E}{R}e^{-\frac{S}{2\varepsilon}t}$$

instead of

$$\frac{E}{R}e^{-\frac{S}{L}t}$$

when it was open.

Hence the initial or maximum rate of variation of the battery-current either at make or break when the secondary current is closed, is to its initial rate of variation when the secondary is unclosed as L is to $2(L-M)$,—a ratio which, in the case supposed, is nearly infinite.

The Theory of Tertiary Currents as produced in an induction-balance—that is, in a coil which is arranged so as to be conjugate to the primary coil, but which is in the neighbourhood of a third coil or conductor affected inductively by the primary current.

16. This is the case of the induction-balance disturbed by a coin. The primary and secondary circuits are arranged in some conjugate position so that they have no effect on each other—this being done practically by splitting each into two equal parts, and arranging the two parts so as to oppose each other; and the third circuit is represented by the piece of metal put near or inside the primary and secondary coils.

The primary circuit contains as part of itself a battery and an intermitter; the secondary contains a telephone.

Neglecting electrostatic capacity and leakage (which latter, by the way, must be carefully avoided in a good balance), the current in either primary is the same, and the current in each secondary is the same, not only on the whole, but at every instant of time. In fact, the separation of the two halves of the primary and secondary is immaterial to the theory. The current which is induced in the secondary circuit, and which is heard in the telephone, is a tertiary current produced by induction from the third coil or piece of conducting matter.

The currents in the battery-circuit at any instant being called i, that induced in the metal being k, and that induced by this in the secondary circuit being called j, and being the value appreciated by the telephone, the equations for determining these three quantities are given in § 5, on the supposition that the coefficients of mutual induction between the metal and the two coils respectively (m and μ) are small numbers, which is always true in practice.

The battery-circuit is then practically undisturbed by the presence of the other parts of the instrument, and may be regarded as in space by itself (see end of §§ 12 and 13); so that all we have said about the intermittence of currents in such a circuit in sects. 6–11 applies to the primary of an induction-balance: we will, however, only take the case when the resistance jumps suddenly from any finite value R to another finite value S. The value of i is then given by equation (4); and its rate of variation is

$$\frac{di}{dt}=-\frac{\mathrm{E}(\mathrm{S}-\mathrm{R})}{\mathrm{RL}}e^{-\frac{\mathrm{S}}{\mathrm{L}}t};$$

but as (equation 2)

$$\lambda\frac{dk}{dt}+\rho k+\mu\frac{di}{dt}=0,$$

it is easy to determine k; viz.

$$k=\frac{\mu\mathrm{E}(\mathrm{S}-\mathrm{R})}{\mathrm{R}(\mathrm{L}\rho-\mathrm{S}\lambda)}\left\{e^{-\frac{\mathrm{S}}{\mathrm{L}}t}-e^{-\frac{\rho}{\lambda}t}\right\}. \quad . \quad . \qquad (39)$$

This value agrees exactly with that obtained in the general case (§ 12) when the condition that M^2 might be neglected was put in (see eq. 28), and therefore confirms the correctness of our working. All that was said about the special cases of j in § 13 is therefore now true of k. What we want to find, however, is not k but j, the current induced in the telephonic circuit; hence, differentiating the above expression for k and substituting it in

$$l\frac{dj}{dt}+rj+m\frac{dk}{dt}=0,$$

we get, remembering the initial condition that $j=0$ when $t=0$,

$$j=-\frac{m\mu\mathrm{E}(\mathrm{S}-\mathrm{R})}{\mathrm{R}}\left\{\frac{r}{(\mathrm{L}r-l\mathrm{S})(l\rho-\lambda r)}e^{-\frac{r}{l}t}\right.$$

$$\left.+\frac{\rho}{(l\rho-\lambda r)(\lambda\mathrm{S}-\mathrm{L}\rho)}e^{-\frac{\rho}{\lambda}t}+\frac{\mathrm{S}}{(\lambda\mathrm{S}-\mathrm{L}\rho)(\mathrm{L}r-l\mathrm{S})}e^{-\frac{\mathrm{S}}{\mathrm{L}}t}\right\}. \quad (40)$$

And this is the current heard in the telephone.

The expression $\int_0^\infty jdt$ identically vanishes; hence this current cannot affect a galvanometer (see § 1).

At "make" $\mathrm{R}=\infty$, and the three terms in the brackets are all present, the factor outside reducing to $+m\mu\mathrm{E}$. But at "break" $\mathrm{S}=\infty$, and the third term disappears, leaving

$$j=\frac{m\mu\mathrm{E}}{\mathrm{R}(l\rho-\lambda r)}\left\{\frac{r}{l}e^{-\frac{r}{l}t}-\frac{\rho}{\lambda}e^{-\frac{\rho}{\lambda}t}\right\}; \quad . \quad . \quad (41)$$

which is the tertiary current at break, and has some of the characteristics of the secondary current at make, see equation (28).

17. If the battery and telephone circuits are *similar*, so that $\frac{\mathrm{S}}{\mathrm{L}}=\frac{r}{l}$, the value of j at the make simplifies considerably, becoming

$$\left.\frac{m\mu\mathrm{ES}}{\mathrm{L}l(\lambda\mathrm{S}-\mathrm{L}\rho)}\cdot te^{-\frac{r}{l}t}\right\}, \quad . \quad . \quad . \quad . \quad (42)$$

which in this case therefore bears a constant ratio to the secondary current at make under the same circumstances (see equation 31).

Theory of the Induction-balance continued.

18. Having now integrated equations (2) and obtained an expression (40) for the current passing through the telephone at any instant, several things may be noticed about it. First,

the current produced by the piece of metal is in general different in character to that produced by a slight shift of the secondary coil (that is, by M being made not quite zero); and consequently it is impossible in general to balance the effect of metal completely by moving the other pair of coils a little further apart, though it may be done partially. Both effects would then be superposed, and the two sounds would be separately heard and might differ in pitch, because there would be more oscillations in the tertiary current than in the secondary. It is quite possible that a musical ear would perceive that the quasi-note produced by the insertion of a piece of metal contains a trace of a tone an octave above that produced by a shift of the coils. Equation (42), however, appears to show that when the battery- and telephone-circuits are *similar* the secondary and tertiary currents are expressible by the same function of the time, and that, therefore, in this case the effect of introducing metal *may* be counteracted by a shifting of the coils.

Next, we see that the effect is independent of M (that is, of the mutual induction between the primary and secondary coils); and therefore it may occasionally be well to place them in some nearly conjugate position, so as more easily to obtain a balance. In fact the four coils are unnecessary; two coils placed so as not to act on each other will do; and a piece of metal placed near them will produce disturbance; only it will not be so easy to bring the metal close or parallel to both coils.

Next the effect is proportional to the produced $m\mu$; of which μ is the potential of the primary, and m that of the secondary, on the coin. Hence, if the two coils are equal in size, every thing is symmetrical with respect to them, so that it does not matter whether the coin is nearest the primary or the secondary; moreover the middle point halfway between the two coils must be either a maximum or a minimum position of the coin.

Prof. Hughes long ago stated, from experimental observation, that the position of the coin which gave the loudest sound was half way between primary and secondary, the two coils being pretty close together and being equal in all respects. Before investigation I had felt inclined to doubt whether this were more than approximately true (as it was by no means evident *à priori* that every thing was symmetrical with respect to both coils, and the middle of the primary coil seemed a not improbable place for the maximum). Accordingly I arranged two pairs of coils as an induction-balance, the components of each pair being 4 or 5 inches apart, but with their planes parallel as usual; and I then moved a coin about along the common axis of one pair to see what happened. The maximum was *not* in the middle; but there were two maxima, one on each side the middle, and every thing appeared symmetrical with respect to both coils. The preceding investigation throws light upon this, and reconciles the two observations. It turns out (as might have been anticipated) that Professor Hughes's statement is quite correct when the coils are near together; but if they are separated by a distance greater than the diameter of either (the coin being supposed small), the middle point becomes a *minimum* with a maximum on each side of it (see next section).

On the Law according to which the disturbance produced by a small coin on the common axis of a pair of coils in an induction-balance depends on its position.

19. The general expression for the induction-coefficient between two circular coils in any position whatever is given in 'Maxwell,' art. 696. We can specialize this to the case required, viz. a coil of n turns and a "coin" or coil of one turn, both on the same axis. Let the mean radius of the "coil" be a, and let the distance of its circumference from some point O on the axis be c. Also let the mean radius of the "coin" be b, and let d be the distance of its centre from the point O. Then the mutual induction-coefficient between the coil and the coin is (art. 699)

$$2n\pi^2\frac{a^2b^2}{c^3}\left\{1+3\frac{\sqrt{(c^2-a^2)}}{c^2}d+6\frac{c^2-\frac{5}{4}a^2}{c^4}(d^2-\tfrac{1}{4}b^2)+\text{\&c.}\right\}.$$

Now d we can make zero at once by taking the point O at the middle of the coin, so that c is the distance from the mean circumference of the coil to the middle of the coin; and there remain inside the brackets of the above expression 1 + terms involving the square and higher powers of $b : c$, which is a small quantity. Thus a very good approximation to the induction-coefficient between the primary and a small coin is

$$\mu = \frac{2\pi^2 a^2 b^2 n}{c^3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (43)$$

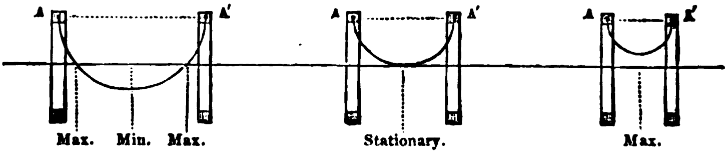
Similarly the induction-coefficient between the secondary and the coin is the same expression with dashed letters; hence the product

$$m\mu = \frac{nn'}{(cc')^3}(2\pi^2 b^2 aa')^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (44)$$

And this is a maximum when cc' is a minimum, a thing which is very easily represented geometrically.

For let A and A′ be points on the mean circumferences of the coils from which c and c' are measured; draw the system of lemniscates cc' = various constants, and then draw through the system any straight line at a distance a from A and a' from A′. This line will represent the common axis of the two coils, and points on it represent possible positions of the coin. Now the line cuts every lemniscate in four points: hence there are four points on the axis at which the coin produces the same disturbance. These can run together two and two at points where the line is touched by the lemniscate; and these are the maximum or minimum points. They are maxima if the line is touched on the side of A A′, but minima if the line is touched on the other side.

If the coils are the same size (*i. e.* if $a = a'$), and if they are at a proper distance apart (viz. $= 2a$), all four points can run together in the middle, which is therefore then a stationary point, so that moving the coin a little either way will produce very little diference. But if the coils are closer together than this, two of the points of section become imaginary, and hence there are now only two positions of the coin which give the same noise; and these two can run together in the middle, which is now a maximum.



Analytically the problem might be stated thus:—Find the max. and min. values of $\sin\theta \sin\theta'$, given the condition

$a \cot\theta + a'.\cot\theta' =$ const., θ and θ' being the angles subtended by mean radii of the primary and secondary coils at the coin. If the coils are equal ($a=a'$), the solutions are $\theta=\theta'$ and $\theta+\theta'=\frac{1}{2}\pi$; hence another way of stating the result is the following:—Draw a circle with A A′ (the mean points of the equal coils) as diameter: then if this circle cuts the common axis of the coils, the points of section are maxima, and the middle point is a minimum; but if the circle does not cut the axis, the middle point is a maximum, and the only one (see figure).

The law of decrease with distance is interesting: equation (44) shows us that when the two coils are close together and the coin is moved along the axis away from them, the effect which it is able to produce in the telephone varies pretty nearly as the inverse *sixth* power of the distance as soon as it has got a little way—a tremendously rapid rate of decrease.

It also shows that the effect varies directly with the fourth power of the diameter of the coin.

The Connexion between Induction-balance Effect and Conductivity.

20. The way in which the telephone-current depends on the conductivity $\frac{1}{\rho}$ of the coin is apparent in equation (40): it is evidently not simply proportional to it in general. But the self-induction λ of a circuit like that in a solid disk of metal must always be a very small quantity; and, except perhaps for highly conducting metals, it must probably be almost negligible compared with ρ. Assuming, then, that λ is infinitesimal, the expression simplifies, and ρ becomes a factor of one part of it. We get, in fact,

$$j = \frac{m\mu E(S-R)}{\rho R(Lr - lS)} \left\{ \frac{S}{L} e^{-\frac{S}{L}t} - \frac{r}{l} e^{-\frac{r}{l}t} \right\} + \frac{m\mu E(S-R)}{R(L-l)} e^{-\frac{\rho}{\lambda}t} \quad . \quad . \quad . \quad . \quad (45)$$

The term written by itself does not contain ρ except as an exponential; but it has a very great rate of variation depending directly on ρ, and it soon ceases to exist. The rest of the expression for the current is simply proportional to the conductivity, both as regards its own value and its rate of variation. What the precise meaning of all this is depends on what the loudness of the telephone-indication definitely depends on (§ 1); so for the present we will leave it in this state.

On Measuring with the Induction-balance.

21. Two methods have been employed both by Professor Hughes and by Mr. Roberts, with the view of obtaining quantitative readings from the balance. The first consisted in estimating the loudness of the sound produced by the insertion of a coin, and then imitating it by an arrangement of primary and secondary coils called the "sonometer"—a key being used to transfer the connexions of the telephone quickly from one instrument to the other. Mr. Poynting has adapted a formula from Maxwell which suffices to graduate the sonometer when the secondary coil is some distance from either primary (Phil. Mag. January), though it is rather an unwieldy one.

But there must always be some objection to readings taken in this way, because of the difficulty of estimating precisely when two sounds have the same strength—especially if, as appears probable from § 18, they differ at all in quality, the sound produced by the coin in the balance having a tone mixed up with it which is shriller than any produced by the sonometer. (I do not know whether this has ever been noticed experimentally. One often notices a change of pitch in the telephone-rustle; but it appears to depend in some cases on the direction of the current, *i. e.* on whether it strengthens or weakens the magnet.)

If the sonometer is to be used, I would suggest that the second primary coil be done away with, and that the secondary coil be of small diameter. It may have as much wire on it as is wanted for long range; but the further it is away from the primary, compared with its own diameter, the better. The diameter of the primary does not much matter. The induction would now simply vary as the inverse cube of the distance from the mean circumference of the primary; hence, if the sonometer were graduated so as to give equal distances from this line, its readings would only have to be turned upside down and cubed to give comparative results. If both primary and secondary coils are small, the sonometer-arm may be graduated like an ordinary millimetre-scale.

If an absolute zero were wanted at any time, it might be obtained by an arrangement for rotating one of the coils till it was at right angles to the other.

22. Prof. Hughes's second method of measuring, that of the graduated zinc wedge, is a much better one, because it is a null method and gives true readings, though they are not easily interpretable.

A third method has, I believe, been tried, viz. a copper damper rotating above one pair of coils; and this seems also pretty good; but I think it might possibly be better to modify it by having a uniform disk (or ring) of high-conductivity copper capable of being moved along the axis of one pair of coils with its plane always parallel to theirs. The distance of the disk from the middle point of the coils should be read, and the distance apart of the coils should be constant. Given all particulars, I have no doubt that a mathematician could readily interpret results so obtained.

Another modification would be to use a coil of wire forming a circuit closed within itself instead of the copper disk. It would have just about the same effect as a solid disk of the same size; but the data connected with it might be more definite.

The Effect of Magnetic Bodies.

23. If a thin piece of iron wire be held in the balance, it powerfully disturbs it by a concentration of the lines of force in the iron, so that M (the mutual induction between the battery- and telephone-circuits) is no longer zero. If the iron were instantaneously magnetizable, one ought to be able to balance its effect by moving the other pair of coils a little nearer together, so as to make M zero again. But as the magnetization of iron always takes time, this is not completely possible, because, though M may be made zero at any one instant, it will not remain so during the period of variation of the current. As regards pitch, the rustle produced by iron wire should agree with that produced by a shift of the coils, both being duller than that produced by a piece of copper. Of course if a *mass* of iron be inserted, the effect will be compo-

site, secondary currents being induced in it which give rise to tertiary currents in the telephone-circuit. But this effect is feeble as compared with the magnetic effect.

In measuring the conductivity of a metal or of an electrolytic solution by means of the induction-balance, care must be taken that it is not magnetic; or erroneous results will be obtained. The conductivity effect and the magnetic effect will tend partially to destroy each other; for though the effect of a piece of copper cannot be completely balanced by introducing a scrap of iron into the same pair of coils, it can partially, just as it might partially by shifting the coils (§ 18).

It would be very interesting to observe an opposite effect with a diamagnetic body—say a large bundle of thin bismuth wires insulated from each other; but it would not be easy to make sure of the correctness of the observation.

PROCEEDINGS

OF

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XXIX. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part I. *By* C. R. ALDER WRIGHT, *D.Sc. London, Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School.*

1. BETWEEN the years 1841 and 1846 Joule made a series of experiments on the development of heat during electrolysis, which led him to the conclusions:—that when a current is passed through a fluid conductor, heat is developed in accordance with the same law as that which he found to obtain with a solid metallic wire, viz. that the heat produced in a given time is proportionate to the square of the current and to the resistance of the conductor—usually known as Joule's law; that if electrolysis is produced, the actual heat developed in the electrolyte is less than that deducible from this law by a quantity precisely equal to the amount that would be given out in re-forming from its products of decomposition the compound decomposed; and consequently that it is possible to determine the "heat of formation" of a compound by the inverse method of determining the heat absorbed during its electrolysis. The actual results thus obtained, as regards the "heat of formation" of copper sulphate, zinc sulphate, and water, were communicated to the French Academy in 1846, but were not published in detail until 1852 (Phil. Mag.

[4] vol. iii. p. 481). Some few months before they appeared, Dr. Thomas Woods published (ibid. vol. ii. p. 268) the results of experiments showing that in the decomposition of water by electrolysis a quantity of heat is taken up approximately equal to that evolved by the recombination of the hydrogen and oxygen evolved.

2. Joule's method of operating is described by him in the following words:—"I take a glass vessel filled with the solution of an electrolyte and properly furnished with electrodes; I place the electrolytic cell in the voltaic circuit for a given length of time, and carefully observe the quantity of decomposition and the heat evolved. By the law of Ohm I then ascertain the resistance of a wire capable of obstructing the current equally with the electrolytic cell. Then by the law we have proved [*i. e.* 'Joule's law'] I determine the quantity of heat which would have been evolved had a wire of such resistance been placed in the current instead of the electrolytic cell: this theoretical quantity, being compared with the heat actually evolved in the electrolytic cell, is always found to exceed the latter considerably. The difference between the results evidently gives the quantity of heat absorbed during the electrolysis, and is therefore equivalent to the heat which is due to the reverse chemical combination by combustion or other means" (*loc. cit.* pp. 493, 494).

Put into symbols this may be thus expressed:—Let h_1 be the heat actually developed in the electrolyte by a current C_1 in a given time t during which w_1 grammes of electrolyte are decomposed. Let h_2 be the heat that would be developed in a wire of the same resistance (*i. e.* capable of equally "obstructing the current") in the same time t by the same current C_1. Let h_3 be the heat actually developed in another wire by a different current C_2 in the same time t; and let the resistance of the electrolytic cell be a times the resistance of this second wire. Then, by Joule's law,

$$\frac{h_2}{h_3}=\frac{C_1^2}{C_2^2}a,$$

whence the heat absorbed in decomposing w grammes of electrolyte is

$$h_2-h_1 \text{ or } =h_3\frac{C_1^2}{C_2^2}a-h_1,$$

and the heat absorbed in decomposing 1 gramme of electrolyte is

$$\frac{h_3 \frac{C_1^2}{C_2^2} a - h_1}{w}.$$

In this way Joule obtained in three sets of observations the following mean values for the heat absorbed in the decomposition of a gramme-equivalent of water:—

	34101
	34212
	32356
Average . .	33557

the numbers found by direct determination of the heat of combustion of hydrogen in oxygen being close to 34100 gramme-degrees, according to the most trustworthy experiments (*vide* § 31).

3. Joule further applied this principle to the electrolysis of zinc and copper sulphates, and found that the heat actually developed in the electrolytic cell was in each case less than that which would be developed in a wire "obstructing the current" equally with the cell by a quantity sensibly equal to the heat that would be generated by the oxidation of the metal liberated during the electrolysis by the oxygen simultaneously set free, together with that produced by the solution in the dilute sulphuric acid also formed of the metallic oxide thus produced. That this should be so is an evident deduction from the principle of conservation of energy; for if the total work that could be done in the passage of a current C through a resistance R in a time t be expressed by the formula C^2Rt (Joule's law), and some of that work be performed not as heat but in undoing chemical combination, it results that the amount of work actually done as sensible heat must be just so much less than C^2Rt as corresponds to the chemical work performed.

4. The experiments of Woods were of a far less accurate kind than those of Joule, but were based on much the same principles. A current being passed through an electrolytic cell for a given time, the heat developed was roughly measured and the amount of decomposition; a wire was then substituted for this electrolytic cell of such resistance that the current

passing was the same as before (the electromotor remaining the same). The heat developed in this wire being determined, was found to exceed that produced in the electrolytic cell in the same time by a quantity sensibly equal to that which would be generated by the recombination of the products of electrolysis.

The formula just deduced for Joule's method of experimenting then becomes

$$\frac{h_3 - h_1}{w};$$

for $C_1 = C_2$ and $a = 1$.

5. In 1865 Joule's results were confirmed by Raoult (*Ann. de Chim. et de Phys.* 4th series, ii. p. 317, and iv. p. 392) by a method which will be discussed later on (§ 23). The amount of heat absorbed per gramme-equivalent of water decomposed was found by Raoult to be 34028, or sensibly the same as that deduced by direct combustion; whilst similar results were obtained with copper sulphate. In 1869 Kiechl published experiments (*Wien. Akad. Ber.* lx. [2] 121) in principle the same as Joule's, but differing considerably in the details, notably in that the wire was made into a rheostat, so that its resistance could be regulated throughout the observations. As a mean of nine accordant experiments, 33653 was found to be the heat absorbed per gramme-equivalent of water decomposed. Shortly before this, Favre demonstrated in a very simple way (*Comptes Rendus*, lxvi. p. 252; Pogg. *Ann.* cxxxv. p. 300) that as much heat is absorbed during the decomposition of an electrolyte as is produced by the recombination of the products of decomposition. A small battery was enclosed in a calorimeter, and the heat produced by the solution of a given quantity of zinc determined, the current produced being made to circulate through a coil of wire also contained in the calorimeter; so that the total heat generated was determined. A voltameter was then substituted for the coil of wire, when the quantity of heat was found to be lessened by an amount equal to 34,204 gramme-degrees per gramme-equivalent of zinc dissolved, *i. e.* of water decomposed. Similarly when copper sulphate was decomposed instead of water, the heat-evolution was lessened by an amount sensibly equal to the heat evolved in the synthesis of the salt.

6. In 1851 Sir William Thomson carried the ideas suggested by the experiments of Joule and others a considerable way further. In a remarkable paper on the mechanical theory of electrolysis (Phil. Mag. 1851, vol. ii. p. 429), he demonstrated that the force of chemical affinity is expressible in terms of electromotive force, and calculated, from data largely furnished by Joule, that the work done in decomposing water corresponds to an E.M.F. of 3,305,400 foot-grain-second electromagnetic units (equal to $1\cdot415 \times 10^{8}$ C.G.S. units, or 1·415 volt), that representing the sum of the chemical actions in a Daniell cell corresponding to 2,507,100 (equal to $1\cdot074 \times 10^{8}$ C.G.S. units); so that the "affinity" between oxygen and hydrogen in water is $\frac{1\cdot415}{1\cdot074} = 1\cdot318$ times as strong as the sum of the affinities exerted in the chemical changes going on in a Daniell cell. Similar calculations, based on more recent determinations of the values involved, lead to somewhat higher numbers, approximating to $1\cdot502 \times 10^{8}$ C.G.S. units for water and $1\cdot154 \times 10^{8}$ for the Daniell cell (*vide* § 16), giving the ratio $\frac{1\cdot502}{1\cdot154} = 1\cdot302$.

7. The general proposition that the force of chemical affinity is expressible in terms of E.M.F. may be simply demonstrated as follows:—Let the strength of chemical combination, or the chemical work done by the force of chemical affinity, be defined as the absolute amount of work spent or gained in the synthesis from its constituents of a given weight (say 1 gramme) of the compound considered. This work may be expressed in terms of heat, and is ordinarily measured as such: thus in the synthesis of a gramme of water from gaseous oxygen and hydrogen at the ordinary pressure and temperature, an amount of work is done representing an evolution of a quantity of heat amounting, according to the most accurate determinations, to close upon 3800 gramme-degrees (§ 31); whence, taking the mechanical equivalent of heat as 42 megalergs per gramme-degree*, the work done in this synthesis is close to 16 erg-tens $= 16 \times 10^{10}$ C.G.S. units.

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* The values obtained by Joule by friction of water lie somewhat below this figure (near to $41\cdot5 \times 10^{6}$); whilst that deduced by him from the development of heat by electric currents in a wire of known resistance

The experiments of Faraday first demonstrated the two following laws, which have since been fully confirmed by many other experimenters:—

I. When a compound is electrolyzed, the weight of substance decomposed is proportionate to the quantity of electricity passing. [A low degree of conductivity is supposed by some experimenters to be possessed by electrolytes, of such a nature that very feeble currents can be passed through them without causing any chemical decomposition; this, however, even if admitted to be the case, would not appreciably interfere with the correctness of the foregoing proposition.]

II. When a current is passed for a given time successively through several compounds so as to electrolyze them all, it decomposes quantities of them in the ratio of simple functions of their "molecular weights," evolving the products of decomposition, when elementary, in the ratio of simple functions of their combining numbers or "atomic weights." The quantities of compounds decomposed and of elements evolved by a current which evolves 1 part of hydrogen from water under these circumstances are called their "equivalent values," or, more briefly, their *equivalents*, in reference to the particular compounds decomposed.

Law I. may be thus expressed:—Let n be the number of grammes of compound decomposed, and q the quantity of electricity passing; then

$$n \propto q.$$

Law II. states that, if a be the "equivalent" of a given compound,

$$n \propto a.$$

Hence

$$n \propto aq;$$

and consequently

$$n = aq \times \chi,$$

where χ is a multiplier independent of the quantity of electricity passing, and of the value of the "equivalent" of the compound.

---

lies somewhat above it ($42{\cdot}10 \times 10^6$). Numbers lying somewhat above 42×10^6 have been obtained by Hirn, Violle, and Regnault (*vide* §§ 33 & 34).

When $n=a$, $\chi=\frac{1}{q}$; that is, χ is the reciprocal of the quantity of electricity requisite to decompose a gramme-equivalent of the electrolyte employed. For convenience of reference, χ may be termed the *electrochemical constant.*

8. The term "electrochemical equivalent of a substance" is employed to mean the quantity of substance decomposed by a unit quantity of electricity, *i. e.* the value of $\frac{n}{q}$ (which $=a\chi$); and the term "electrochemical equivalent of an element," in reference to such a compound, is used to indicate the quantity of that element evolved during electrolysis of that compound during the passage of a unit of electricity. If b be the equivalent of the element in reference to the compound employed, the equivalent of the compound being a, the electrochemical equivalent of the element in reference to the compound is

$$\frac{n}{q} \cdot \frac{b}{a},$$

which $=b\chi$.

n is of necessity a constant for any given compound: and so is b in many cases; that is, in such cases, no matter what compound of a particular element be electrolyzed, the same quantity of that element is set free by the passage of a unit of electricity. In other cases, however, b varies with the character of the compound employed; that is, the same quantity of element is not necessarily always evolved by the passage of a unit of electricity. For example, the value of b for hydrogen, chlorine, and silver appears to be constantly 1, 35·5, and 108 respectively; whilst for copper the value is 31·75 for compounds of the class known as cupric salts, and 63·5 for compounds of the class known as cuprous salts. When b is thus variable, its different values are always simple integral multiples of a common divisor.

9. Sir William Thomson calculated in 1851 (*loc. cit. supra*) the value of $a\chi$ for water on the foot-grain-second electromagnetic system to be as follows:—

From data furnished by experiments of	Weber	0·02034
„ „ „	Bunsen	0·02011
„ „ „	„	0·01995
„ „ „	Casselmann	0·02033
„ „ „	„	0·02021
„ „ „	Joule	0·01981
„ „ „	„	0·02030
„ „ „	„	0·02002
	Average	0·02013

On the C.G.S. system (electromagnetic measure) this average value becomes 0·0009282. Stas found that 1 part of hydrogen combines with 7·98 of oxygen to form water, whence $a=8{\cdot}98$ for water, and consequently

$$\chi=\frac{0{\cdot}0009282}{8{\cdot}98}=0{\cdot}00010336.$$

More recent experiments of Kohlrausch (Pogg. *Ann.* cxlix. p. 170) indicate a perceptibly higher value, viz. 0·00010527; for he found that one metre-gramme-second unit of quantity of electricity causes the deposition of 0·11363 gramme of silver, or 1 C.G.S. unit of electricity causes the deposition of 0·011363 gramme of metallic silver; *i. e.* $b\chi=0{\cdot}011363$, whence

$$\chi=\frac{0{\cdot}011363}{107{\cdot}93}=0{\cdot}00010527,$$

since Stas found the equivalent of silver to be 107·93.

In the same paper Kohlrausch gives the following values for the electrochemical equivalent of water deduced from the experiments of Weber, Bunsen, Casselmann, and Joule, allowance being made for probable errors in the values assigned to the horizontal component of terrestrial magnetism (the numbers are given by him in the metre-gramme-second system):—

Weber	0·009396
Casselmann	0·009391
Bunsen	0·009624
Joule	0·009222
Mean	0·009406

On the C.G.S. system this value becomes 0·0009406, whence

$$\chi=\frac{0{\cdot}0009406}{8{\cdot}98}=0{\cdot}00010474.$$

Hence the mean of this value and that deduced by Kohlrausch from his own observations (viz. $\chi=0\cdot00010527$) may be taken as probably the nearest approximation to the truth; *i. e.* $\chi=0\cdot000105005$, or practically $\chi=0\cdot000105$.

10. If it were possible to obtain a current between the two electrodes of a voltameter or other similar electrolytic cell which would decompose the electrolyte without any disturbance of the thermal equilibrium, then the difference of potential between the electrodes would represent the force of chemical affinity exerted between the products of decomposition in the compound examined; for if e be the potential-difference (E.M.F. existing between the electrodes) during the passage of a quantity of electricity q, eq units of work will be done. If n grammes of substance are decomposed, and f is the work done by the force of chemical affinity in the synthesis of a gramme of substance from the products of decomposition, the total work done is also expressed by nf, which, by the definition of electrochemical equivalents (§ 8), $=a\chi qf$. Hence

$$af=\frac{e}{\chi}.$$

Or "affinity," reckoned per gramme-equivalent, is measured by electromotive force divided by a quantity which is constant; *i. e.* the work done in the synthesis of a gramme-equivalent of substance is expressible by a value indicated in electromotive-force units, just as it is expressible by a value indicated in heat units—the one valuation involving the constant χ, the other J.

11. In practice it is impossible to realize this supposition; for experiment shows that, in the passage of electricity between two electrodes, work is done not only as chemical decomposition but also as heat; by determining the amount of the latter, however, it is easy to calculate how much of the total potential-difference existent between the electrodes is due to the chemical affinity of the constituents: thus, if h gramme-degrees are evolved during the decomposition of n grammes of substance of electrochemical equivalent $a\chi$ by the transfer of a quantity of electricity q, and if E represent the total potential-difference existent between the electrodes, the total work done in the electrolytic cell is Eq, of which hJ is done as heat. Hence

$Eq-hJ$ is done as chemical decomposition. Let this be equal to eq, where e is the E.M.F. representing the chemical affinity to be measured; then

$$e=E-\frac{hJ}{q}=E-hJ\frac{a\chi}{n},$$

since, by definition of electrochemical equivalents (§ 8),

$$q=\frac{n}{a\chi}.$$

Finally, therefore,

$$e=E-H\chi J,$$

where

$$H=\frac{ha}{n},$$

i. e. where H is the heat evolved per gramme-equivalent of substance decomposed.

Experiments made in this way (detailed in Part II., *vide* also § 32) have given numbers for the value of e in the case of water closely corresponding with the value of e deducible from direct combustion-experiments by the formula arrived at above (§ 10), viz.

$$af=\frac{e}{\chi},$$

which becomes

$$e=af\chi=ah'J\chi=H'\chi J,$$

where h' is the heat liberated per gramme, and H' that per gramme-equivalent of water formed by combustion of hydrogen and oxygen.

12. On attentively considering the phenomena attendant on electrolysis, it is evident that it by no means follows that the direct action of the current causes the entire change taking place; physical changes of state and chemical actions secondary to those due to the action of this current alone may occur; so that the value of e thus deduced, although representing the E.M.F. corresponding to the *nett work done* in the sum of the total changes taking place, and consequently representing the "affinity" between the ultimate products in the original compound as above defined, *is not necessarily a measure of the tendency to combine together of the substances primarily formed by the electrolytic decomposition.* For instance, suppose that in

the electrolysis of water the primary products are ordinary gaseous hydrogen and ozone, the latter becoming subsequently transformed (by a secondary chemical action) into ordinary gaseous oxygen: then, since heat is evolved in the conversion of ozone into oxygen to the extent of 14,800 gramme-degrees per 24 grammes of oxygen (Berthelot, *Bull. Soc. Chim. Paris*, 1876, vol. xxvi. p. 56), or 4933 gramme-degrees per 8 grammes of oxygen (corresponding to 9 grammes of water), the value of e deduced as just indicated will be below the true value by an amount equal to $\frac{4933}{34100} = 14{\cdot}5$ per cent.; for the negative term in the expression $E - H\chi J$ will have been overestimated by that quantity.

Again, suppose that the water is brought by a secondary physical action into a quasi-vaporous state before it is decomposed by the current into gaseous hydrogen and oxygen (*e. g.* on the molecular theory of the constitution of matter, suppose that only detached molecules of water are operated on by the current, and not the water *en masse* in the liquid state); since about 600 gramme-degrees are required to volatilize a gramme of water at the ordinary temperature (the latent heat of steam at near 15° being about 600), or 5400 per gramme-equivalent, the value of e deduced as above will be too large by about $\frac{5400}{34100} = 15{\cdot}8$ per cent, because in this case the negative term $-H\chi J$ will be underestimated to that extent.

Consequently, if both secondary actions be supposed to take place (*i. e.* if the electrolysis proper takes place between detached molecules of water converting them into gaseous hydrogen and ozone), the value of e will be $15{\cdot}8 - 14{\cdot}5$, or 1·3 per cent. too large.

Suppose, again, that the primary electrolytic action is to convert liquid water into *liquid* oxygen and hydrogen, and that the conversion of these liquefied elements into the ordinary gaseous condition is due to secondary physical changes; it is evident that the value of e found as above is too large by $\frac{h_1}{34100} \times 100$ per cent., where h_1 represents the heat that is absorbed in the vaporization of a gramme-equivalent of each of the liquefied gases taken together.

Yet again, in the electrolysis of copper sulphate, suppose that whilst the ultimate chemical change is expressed by the equation

$$CuSO_4 + H_2O = Cu + O + H_2SO_4,$$

this change is actually the sum of two changes, of which the first is secondary:—

$$\begin{aligned} &(1)\quad CuSO_4 + H_2O = CuO + H_2SO_4, \\ &(2)\quad CuO \qquad\qquad = Cu + O; \end{aligned}$$

it is evident that the value of e deduced as above will be greater than that corresponding to the second change only, by an amount equivalent to the heat absorbed during the first change.

13. During the last three or four years the writer has made various experiments with the view of determining the E.M.F. measuring the "affinity" between various constituents in different compounds (especially oxygen and hydrogen in water), not only when the substances developed by the electrolysis are in their ordinary conditions, but also when they are "nascent" —*i. e.* with the view of determining, first, the E.M.F. corresponding to the "heat of formation" of (for instance) water from gaseous oxygen and hydrogen, and, secondly, that requisite to produce electrolysis proper *apart from secondary physical and chemical changes.* It is evident that, if such determinations are practicable, the comparison of the two sets of values might lead to interesting conclusions as to the nature of these secondary actions, and might throw light on the internal mechanism of the chemical changes taking place, and especially on the phenomena connected with the "nascent state." Results in this direction have already been obtained by Favre, with this difference, that the values are expressed in gramme-degrees, being obtained entirely by calorimetric observations. The discussion of the observations of this kind already made by previous experimenters, and of the writer's results (obtained chiefly by electrical measurements), will be resumed in a future paper, the present essay having chiefly reference to the determination of the E.M.F. representing the sum of the physical and chemical actions taking place during electrolysis. It should, however, be noticed in connexion with this subject, that since Sir William Thomson first sketched out the Mechanical Theory of Electrolysis (*loc. cit. supra*), more exact measurements have shown that the E.M.F. actually capable of being generated by a given voltaic combination is by no

means necessarily equal to the sum of the physical and chemical changes taking place in the pile, and that it is not necessarily constant for the same ultimate chemical change—varying with the temperature, the state of dilution of the solutions used, &c.

14. For example, when zinc is dissolved in dilute sulphuric acid, the total ultimate change is expressed by the equation

$$H_2SO_4 + Zn = H_2 + ZnSO_4,$$

which corresponds to an evolution of somewhat more than 18,000 gramme-degrees per gramme-equivalent of zinc dissolved or of hydrogen liberated, and representing therefore about $18000 \times 0{\cdot}000105 \times 42 \times 10^6 = 0{\cdot}794 \times 10^8$ C.G.S. units of E.M.F., or about 0·794 volt. Similarly, when zinc or any analogous metal is dissolved in any other acid (*e. g.* hydrochloric) with evolution of hydrogen by a parallel reaction, a heat-evolution of h gramme-degrees results per gramme-equivalent, corresponding to $h\chi J$ C.G.S. units of E.M.F. But when this change takes place in an electromotor with a second metal as the other plate, the E.M.F. actually set up by the tendency to produce this ultimate change varies with the nature of the second metal; and whilst the E.M.F. produced is rarely, if ever, precisely equal to $h\chi J$ even at first, when a current has been made to circulate for some short time the E.M.F. is found to have diminished to an amount depending on the rate of evolution of hydrogen on the surface of the other plate, the character of its surface, the temperature, &c. This is usually expressed by saying that the plates become "polarized," or that a "counter electromotive force" x is set up, diminishing the effective E.M.F. of the arrangement from E, which it was at first, to $E - x$, the value of x varying with circumstances.

From the principle of the conservation of energy it results that, when the current circulates, the difference between the work $h\chi J \times Q$ which would be performed during the passage of a quantity of electricity Q sufficient to decompose a gramme-equivalent of substance, and $(E - x)Q$ the work which the current can actually perform, must make its appearance as sensible heat. If, therefore, $E - x < h\chi J$, the electromotor must rise in temperature from this cause—irrespective of the rise due to the fact that, as the current traverses the electromotive equally with the rest of the circuit, heat is developed therein in proportion to its resistance in accordance with Joule's law (§ 1). If, however, $E - x > h\chi J$, the battery is of necessity proportionately cooled; and if the experiment is so carried out that the resistance of the battery is small compared with that of the rest of the circuit, so that the heat developed in the battery in accordance with Joule's law is but small, the cooling effect may be made to preponderate over the heating effect, so that an absorption of heat on the whole takes place in the battery.

15. This effect has been experimentally studied by Favre (*Comptes Rendus*, lxvi. p. 252, lxvii. p. 1012, lxviii. p. 1300, and lxix. p. 34), who has shown that under the last-named conditions an absorption of heat takes place during the solution of zinc or cadmium in hydrochloric acid, whilst heat-evolution takes place if sulphuric acid be substituted for hydrochloric. In other words, the secondary actions set up, as a whole, absorb heat in the first case and evolve it in the second. Similarly in two-fluid batteries, such as Daniell's and Grove's, analogous results were obtained, although in these cases "polarization" by evolution of hydrogen could not possibly be produced. With cells after Daniell's construction heat was found to be evolved by the secondary actions, though not to a very large extent (about 4·3 per cent. of the heat due to the consumption of zinc and precipitation of copper); whilst with a Grove's battery a considerably larger heat-absorption took place, amounting to about 11·9 per cent., of the heat due to solution of zinc and reduction of nitric acid.

16. This result with a Daniell cell is borne out by experiments of a different kind. The E.M.F. which a Daniell cell is practically capable of giving has been variously estimated by different observers, at from 1·05 to 1·14 volt, 1·10 to 1·12 being a usual average. The E.M.F. corresponding to the heat evolved in the reaction

$$CuSO_4 + Zn = Cu + ZnSO_4$$

(which expresses the ultimate change in a Daniell cell, apart from local action) is several per cent. higher. Thus Favre estimated the heat thus evolved per gramme-equivalent to be 27,388 (*Comptes Rendus*, lxiii. p. 369), and subsequently as 25,060 (*ibid.* lxix. p. 35): the mean of these two numbers is

26,224, corresponding to an E.M.F. of $26{,}224 \times 0{\cdot}000105 \times 42 \times 10^6 = 1{\cdot}156 \times 10^8$ C.G.S. units, or 1·156 volt. Julius Thomsen has calculated (*Berichte deut. chem. Ges.* 1872, vol. v. p. 170) from his own determinations, together with those of Andrews, Dulong, Hess, and Favre and Silbermann, that the heat-evolutions taking place during the formation of zinc sulphate and copper sulphate are respectively 108,460 and 56,216 per "gramme-molecule" (these formations being expressed in Thomsen's symbols thus—Zn, O, SO_3, aq, and Cu, O, SO_3, aq). Hence the heats are 54,230 and 28,108 per gramme-equivalent; whence the displacement of copper from copper sulphate by metallic zinc, forming copper and zinc sulphate, must give rise to an evolution of $54{,}230 - 28{,}108 = 26{,}122$ gramme-degrees per gramme-equivalent, corresponding to $26{,}122 \times \chi J = 1{\cdot}152$ volt.

17. Results analogous to those of Favre have also been obtained by Raoult (*Ann. de Chim. et de Phys.* 4th series, ii. p. 317, and iv. p. 392)—some forms of voltaic combinations giving rise to the production of more heat outside the battery than corresponds to the heat evolved by the ultimate chemical and physical changes taking place therein, and others to less. Raoult found that the current from the Daniell cell used by him was capable of doing total work equal to 23,900 gramme-degrees per gramme-equivalent of zinc dissolved in each cell: this represents only $23{,}900 \times \chi J = 1{\cdot}054$ volt, a quantity less by 8·5 per cent. than the value calculated above from Thomsen's figures as the E.M.F. corresponding to the sum of the chemical and physical actions taking place in the cell.

To express the difference between the heat actually producible by the sum of the chemical and physical actions taking place in an electromotor and that corresponding to the current actually obtainable from the electromotor, Raoult calls the first the *chemical heat* of the pile, and the latter the *voltaic heat* thereof; whilst the terms *chemical heat* and *galvanic heat* have been employed by Edlund to indicate these quantities respectively (Phil. Mag. 1877, vol. iii. pp. 428 & 501); so that the above results may be indicated by saying that the chemical heat of a Daniell battery is 26,122 gramme-degrees (per gramme-equivalent of zinc dissolved), whilst the voltaic or galvanic heat is 23,900, and so on. Several points touched upon in these papers are of interest as bearing on the problem of the determination of the amount of E.M.F. corresponding to the primary effects of electrolysis; their discussion, like that of several analogous memoirs by previous observers, is postponed till a later occasion.

18. In order to apply the general method above indicated (§ 11) for the determination of the E.M.F. corresponding to the sum of the chemical and physical actions taking place during electrolysis, it is requisite to determine:—first, the difference of potential subsisting between the electrodes; secondly, the quantity of electricity passing in a given time (known from the weight of substance decomposed); and, thirdly, the heat evolved as such in the electrolytic cell in that time. The first quantity may be found approximately by the aid of a sine galvanometer of very high resistance. A method which is in principle substantially this has already been used by Raoult (*loc. cit.*); this will be more fully discussed further on (§ 23).

Another way of determining it may be based upon observations with a voltameter, thus:—The amount of decomposition produced in a given time t by passing a current from the battery employed (of E.M.F. $= E'$) through the voltameter is noticed $= n_1$; the electrolytic cell is then placed also in circuit, and the amount of decomposition in the voltameter in the same time t again determined $= n_2$, and also the heat produced in the cell $= h$. R being the resistance of the battery, voltameter, and connexions, and r being the resistance of a wire "obstructing the current equally with the electrolytic cell," the currents flowing in the two cases are respectively

$$C_1 = n_1 \times k = \frac{E'}{R} \text{ and } C_2 = n_2 \times k = \frac{E'}{R+r},$$

where k is a constant—whence

$$\frac{R}{R+r} = \frac{n_2}{n_1}, \text{ and } \frac{r}{R+r} = \frac{n_1 - n_2}{n_1}.$$

The difference of potential set up between two points connected by a medium of resistance r traversed by a current C_2 is

$$rC_2 = \frac{r}{R+r} E' = \frac{n_1 - n_2}{n_1} E';$$

and hence the expression for the E.M.F. equivalent to the affinity to be found, $E - H\chi J$, becomes

$$\frac{n_1 - n_2}{n_1} E' - \frac{ha\chi J}{n_2},$$

where a is the equivalent of water $= 8{\cdot}98$.

19. Experiments on the electrolysis of water have been made by the writer in accordance with this method, and have furnished moderately concordant results approximating to the value of the E.M.F. representing the affinity to be measured, calculated from the heat of combustion of hydrogen (§ 32); but as the experimental errors attendant on the method are considerably greater than those involved when the value of $\frac{r}{R+r} E'$ is directly determined by means of a quadrant electrometer, this latter mode of operating has been adopted in preference: the details of observations thus made in the case of water are given in Part II. Meanwhile it is of interest to notice that if a tangent galvanometer be substituted for a voltameter (the quantity of electricity passing being determined by observing the quantity of electrolyte w decomposed whilst a quantity of heat h is evolved), the value of the affinity to be determined is given by the analogous formula

$$e = \frac{\tan\alpha - \tan\beta}{\tan\alpha} E' - \frac{ha\chi J}{w},$$

where α and β are the deflections observed with the currents C_1 and C_2 respectively. The experiments of Joule above referred to (Phil. Mag. vol. iii. p. 481), although made for the purpose of calculating the heat of formation of water, copper sulphate, and zinc sulphate in a wholly different way, contain the data requisite for the application of this mode of observation, based on the use of the tangent galvanometer; it being taken for granted that E′, the value of the E.M.F. of the Daniell battery used, was $m \times 1{\cdot}10$ volt, where m is the number of cells used in the experiment recalculated. Thus, Joule found the following numbers in a series of four observations on the electrolysis of water in which six Daniell cells were used:—

	Mean tan α.	Mean tan β.	Gramme of H evolved.	Rise of thermometer.
	3·4170	1·6234	0·03978	40·6 scale divisions.
	3·9429	1·7780	0·04212	37·8 ” ”
	4·3412	1·8374	0·04372	41·7 ” ”
	4·0508	1·8011	0·04411	38·8 ” ”
Average ……	3·9380	1·7600	0·04243	39·725
		Correction	for radiation…	+ 0·656
				40·381

The water-equivalent of the calorimeter was found to be 1155 grammes, whilst 23·38 scale-divisions of the thermometer represented 1°; hence

$$h = 1994{\cdot}9.$$

The E.M.F. of affinity to be found, then, is

$$\frac{3{\cdot}9380 - 1{\cdot}7600}{3{\cdot}9380} \times 6 \times 1{\cdot}10 \times 10^8$$

$$- \frac{1994{\cdot}9 \times 1 \times 0{\cdot}000105 \times 42 \times 10^6}{0{\cdot}04243},$$

or

$$1{\cdot}577 \times 10^8 \text{ C.G.S. units} = 1{\cdot}577 \text{ volt.}$$

The value deduced from Joule's own figures by his mode of experimenting is 34,101 gramme-degrees per gramme-equivalent of water decomposed, representing $34101 \times \chi J = 1{\cdot}504$ volt; whilst the value deducible from the best determinations of the heat evolved in the combustion of hydrogen is 34,100, representing 1·504 volt (§ 32). In two other similar sets of observations, Joule obtained the values 34,212 gramme-degrees corresponding to 1·509 volt, and 32,356 gramme-degrees corresponding to 1·427 volt. Unfortunately the data for recalculating these values, as has just been done with the first set, are not given in his paper; still the difference between the values of the E.M.F. of affinity, calculated as above and from Joule's heat-values, is apparently no greater than the differences observed in these latter on repetition. It is noticeable that a small error in the value of the E.M.F. of the battery produces a much larger error in that of the calculated result: thus, if in the set of observations recalculated as above the E.M.F. of a Daniell cell be taken as 1·08 instead of 1·10 volt, the calculated E.M.F. of affinity comes out 1·511 instead of 1·577 volt; *i. e.* a diminution of less than 2 per cent. in the value of the Daniell cell makes a diminution of more than 4 per cent. in the E.M.F. of affinity.

20. In just the same way Joule's experiments on copper sulphate and zinc sulphate may be applied by means of the above formula. Thus with copper sulphate the following numbers were obtained, a battery of 4 Daniell cells being used:—

	Mean tan α.	Mean tan β.	Gramme of copper deposited.	Rise of thermometer.
	3·4006	0·7275	0·5686	20·4
	3·7510	0·7535	0·5777	19·4
	3·8538	0·7650	0·5881	19·45
	3·8084	0·7968	0·6153	17·4
Average	3·7035	0·7607	0·5874	19·162
			Correction for radiation ...	−0·049
				19·113

In these experiments the water-equivalent of the calorimeter was 1179·2, whence $h = 963{\cdot}99$,

and the E.M.F. representing the total chemical and physical changes is

$$\frac{3{\cdot}7035 - 0{\cdot}7607}{3{\cdot}7035} \times 4 \times 1{\cdot}10 \times 10^8 - \frac{963{\cdot}99 \times 31{\cdot}75 \times \chi J}{0{\cdot}5874},$$

or $$1{\cdot}198 \times 10^8 + 10^8 \text{ C.G.S. units or } 1{\cdot}198 \text{ volt.}$$

With zinc sulphate the following numbers were obtained, a battery of 7 Daniell cells being used:—

	Mean tan α.	Mean tan β.	Gramme of zinc deposited.	Rise of thermometer.
	3·5500	0·7167	0·5797	32·2
	4·0133	0·7092	0·5647	30·0
	3·8356	0·7573	0·6010	31·3
	3·9025	0·7548	0·5991	30·5
Average	3·8254	0·7345	0·5861	31·0
			Correction for radiation ...	− 0·016
				30·984

In these experiments the water-equivalent of the calorimeter was 1180·9, whence $h = 1565$,

and the E.M.F. to be found is

$$\frac{3{\cdot}8254 - 0{\cdot}7345}{3{\cdot}8254} \times 7 \times 1{\cdot}10 \times 10^8 - \frac{1565 \times 32{\cdot}5 \times \chi J}{0{\cdot}5861},$$

or

$$2{\cdot}395 \times 10^8 \text{ C.G.S. units}, = 2{\cdot}395 \text{ volts}.$$

21. These numbers 1·198 and 2·395 volts lie close to those calculated from Joule's figures by his own method of experimenting, and also to those calculated from the heat evolved in the synthesis of copper and zinc sulphates from the metal, oxygen, and dilute sulphuric acid; these latter quantities were found by Thomsen (*Deut. chem. Ges. Berichte*, 1872, vol. v. p. 170) to be respectively 56,216 and 108,460 per "gramme-molecule," or 28,108 and 54,230 per gramme-equivalent. A somewhat smaller number than the former of these (viz. 26,568) was found by Favre for the "heat of formation" of copper sulphate from copper, oxygen, and dilute sulphuric acid by the method referred to in § 5. These various results may be thus contrasted:—

Observers furnishing the principal data for calculation.	Nature of method of calculation.	Heat evolved.		E.M.F. of affinity in volts (1 volt $= 10^8$ C.G.S. units).
		Per gramme of metal deposited.	Per grm.-equivalent of metal deposited.	
		Copper Sulphate.		
Joule	Joule's own method, described in §§ 2 & 3.	836·4	26556	1·171
"	"	856	27178	1·199
"	"	796·5	25289	1·115
		829·6	26341	1·162
Joule	From formula $e = E - H\chi J$ (§ 11).	......	......	1·198
Julius Thomsen	From formula $e = H'\chi J$ (§§ 10 & 11).	......	28108	1·240
Favre..............	"	......	26568	1·172
		Zinc Sulphate.		
Joule	Joule's own method (§§ 2 & 3).	1523·1	49501	2·183
"	"	1547	50277	2·217
"	"	1619	52617	2·320
		1563·0	50798	2·240
Joule	From formula $e = E - H\chi J$ (§ 11).	......	......	2·395
Julius Thomsen	From formula $e = H'\chi J$ (§§ 10 & 11).	......	54230	2·392

22. In reference to Julius Thomsen's values, it is noticeable that some slight uncertainty in the precise values of the heat-developments in the syntheses Cu, O, SO_3 aq., and Zn, O, SO_3 aq. is introduced by the circumstance that these values are calculated from determinations of the heat evolved by the direct combustion of metal to oxide, and that produced by solution of the metallic oxide in dilute sulphuric acid—these determinations being made by different observers necessarily working with different specimens of metallic oxides, and their results being averaged. Ditte has shown, however (*Comptes Rendus*, lxxii. pp. 762 & 878, and lxxiii. p. 108), that the heat developed by dissolving a metallic oxide in an acid varies with the degree of heat to which the oxide has previously been exposed. Thus, with zinc oxide and dilute sulphuric acid, the following values were obtained per gramme-equivalent:—

	Gramme-degrees.
Zinc oxide previously heated to 350°	9,890
„ „ „ dark red heat .	11,016
„ „ „ bright „ .	12,138

The value of this "heat of solution" was given by Favre as 10,455, and by Julius Thomsen as 11,705, the temperature to which the zinc oxide had been previously heated not being stated.

23. The experiments of Raoult above adverted to (§ 18) were conducted as follows:—A current from a Daniell battery was passed through a galvanometer and a voltameter placed in a calorimeter, a derived circuit containing a sine galvanometer being also applied to the voltameter-plates; so that the current in this derived circuit virtually measured the difference of potential subsisting between the voltameter-plates. Calling f the "intensity of the derived current," and d that due to a Daniell cell working through the same resistance, and taking 23,900 as the "galvanic heat" (§ 17) of each Daniell cell used, the total work done in the voltameter per gramme-equivalent will be represented by $\frac{f}{d} \times 23{,}900$ gramme-degrees: subtracting this from the heat actually developed as such, a value in gramme-degrees is obtained representing the work done as chemical decomposition. In this way Raoult ob-

tained the following values for the chemical decomposition of water and copper sulphate:—

	Water.		Copper sulphate.	
	33,858	gramme-degrees.	30,407	gramme-degrees.
	34,229	” ”	29,765	” ”
	33,998	” ”	29,683	” ”
Mean .	34,028	” ”	29,951	” ”

These mean values correspond respectively to 34,028 $\chi J =$ 1·501 volt and 29,951 $\chi J = 1·321$ volt. The former number closely agrees with the above-cited results of Joule, Kiechl, and Favre, and with the number deduced by direct combustion of hydrogen (§ 32); the latter is somewhat larger than the values deducible from the observations of Joule, Julius Thomsen, and Favre, given in § 21.

The mode of calculation employed by Raoult, however, was far less simple than would appear from this brief account of his results. He complicated the work by attempts to measure the amount of polarization set up in the voltameter; and from the numbers thus obtained, together with the strengths of the derived currents and the amounts of heat actually generated as such, he calculated certain amounts of heat, to which he applied the terms "local heat" and "voltaic heat of the voltameter," the difference between which represented the heat equivalent to the chemical decomposition—*i. e.* the above numbers. On revising his formulæ, however, it becomes evident that the determination of the amount of polarization of the voltameter-plates (a determination the accuracy of which is open to some criticism) is in no way involved in the determination of the heat equivalent to the chemical decomposition, inasmuch as the terms involving the amount of polarization in the expressions for the "local heat" and the "voltaic heat of the voltameter" are identical, and consequently become both eliminated in the expressions representing the difference between these two quantities—*i. e.* the chemical decomposition.

24. It is evident that any method that will give a direct valuation by electrical measurements of the work done in the formation of a given compound in terms of E.M.F. will, when compared with the direct determination of the "heat of

formation" of the same compound by means of the calorimeter, enable a new valuation to be deduced for J, the mechanical equivalent of heat; for the work done in the formation of a gramme-equivalent of the compound being found by the one method to be measured by e C.G.S. units, and by the other by h gramme-degrees, the relationship

$$hJ = eQ = \frac{e}{\chi}$$

holds, where Q is the quantity of electricity requisite to decompose a gramme-equivalent of substance, *i. e.* where $Q = \frac{1}{\chi}$ (§ 7); whence

$$J = \frac{e}{h\chi}.$$

If, however, the value of J is included in the value of e, as is necessarily the case when e is calculated by the formula $e = E - H\chi J$ (§ 11), this formula becomes

$$hJ = \frac{E - H\chi J}{\chi},$$

whence

$$J = \frac{E}{(H+h)\chi}.$$

25. In the case of water, a number of determinations of h (the heat evolved by the union of oxygen and hydrogen by combustion) have been made by various observers, but for the most part with slightly different values for the heat unit. Leaving out of sight the first approximate determinations of Dulong, Hess, and Grassi (respectively 34,743, 34,792, and 34,666 gramme-degrees per 9 grammes of water formed), the following comparisons of these different determinations may be made, reducing them all, as far as possible, to the same conditions—viz. supposing the oxygen and hydrogen originally used per gramme-equivalent of water (respectively 7·98 and 1 gramme) and the water produced (8·98 grammes) to be all at 0° and 760 millims., the value of a gramme-degree being defined as the heat required to raise 1 gramme of water from 0° to 1° C.

The most recent of these determinations is that of Schuller and Wartha. By introducing hydrogen and oxygen at 0° into a slightly modified Bunsen's ice-calorimeter, burning them, and weighing accurately the water formed, these observers found (Pogg. *Ann.* [2] ii. p. 359), as the mean of four closely concordant valuations, that when 8·98 grammes of water are produced from 1 gramme of hydrogen and 7·98 of oxygen, the materials and products formed being all at 0° and 760 millims., the heat produced is sufficient to raise 341·261 grammes of water from 0° to 100°; *i. e.* defining what they term a "mean calory" to be $\frac{1}{100}$ of the heat required to raise a gramme of water from 0° to 100°, the heat developed is 34126·1 "mean calories." According to Regnault (*Mémoires Acad. Sciences*, 1847, vol. xxi.), the specific heat of water at various temperatures is given by the formulæ

$$Q = t + 0{\cdot}00002\,t^2 + 0{\cdot}0000003\,t^3,$$
$$S = 1 + 0{\cdot}00004\,t + 0{\cdot}0000009\,t^2;$$

where Q is the quantity of heat required to raise a unit of weight of water from 0 to t, and S is the specific heat at t°; so that the mean specific heat between 0° and 100° (*i. e.* the value of a "mean calory") is 1·005. On the other hand, Bosscha has revised the data on which Regnault founded his formulæ (Pogg. *Ann.* Jubelbd. p. 549), and gives the two following expressions for the quantities Q and S:—

$$Q = t + 0{\cdot}00011\,t^2,$$
$$S = 1 + 0{\cdot}00022\,t;$$

whence the mean specific heat between 0° and 100° is 1·011, a value largely exceeding Regnault's. Employing Regnault's value, the heat-evolution observed by Schuller and Wartha becomes 34126·1 × 1·005 = 34,297 gramme-degrees; whilst if Bosscha's formula be employed (as was done by Schuller and Wartha in comparing their results with those of other observers), the heat evolution becomes 34126·1 × 1·011 = 34,501 gramme-degrees.

26. Shortly before these experiments were published, von Than found, by nearly the same mode of operating, that 1 gramme of hydrogen and 7·98 of oxygen, at 0° and 760 millims., liberate, in forming water, 33,928 gramme-degrees (*Deut. chem. Ges. Ber.* 1877, vol. x. p. 947). The chief difference between his experiments and those of Schuller and Wartha was that, whilst the latter weighed the mercury

sucked into the calorimeter through the melting of the ice, von Than measured it in a capillary tube according to Bunsen's plan, using Bunsen's value for a gramme-degree as above defined in terms of mercury sucked in. Schuller and Wartha calculate, from Bunsen's data, that the quantity of mercury corresponding to a "mean calory" is 0·01541 gramme, whilst their own determinations give 0·015442; von Than used values corresponding to the former number, whilst Schuller and Wartha employed the latter. The differences between each of the values used and the true value are included in the experimental errors of determination in the two heat-evolutions—viz. 34,297 (Regnault) or 34,501 (Bosscha), found by Schuller and Wartha, and 33,928 observed by von Than.

27. A few years previously Julius Thomsen found (Pogg. *Ann.* 1873, vol. cxlviii. p. 368), from three accordant observations, that the formation of 17·96 grammes of water from dry oxygen and hydrogen, all materials and products being at near to 18°, produces an evolution of heat sufficient to raise the temperature of 68,207 grammes of water 1°, the water being to start with at close to 18°; the heat required to raise the temperature of the water produced being allowed for in the determination. From Regnault's formula (§ 25) the specific heat of water at 18° is 1·00101, and from Bosscha's 1·00396; so that this observed heat-evolution corresponds to $34103{\cdot}5 \times 1{\cdot}00101 = 34138$, or to $34103{\cdot}5 \times 1{\cdot}00396 = 34239$ gramme-degrees per 8·98 grammes of water, according as the specific heat of water is calculated from the one or the other formula. If, however, the combination of the oxygen and hydrogen took place at 0° (producing water at 0°) instead of at 18° throughout, the amount of heat-evolution would be increased by $a-b$; where a is the heat given out by 8·98 grammes of water in cooling from 18° to 0°, and b the amount of heat required to raise 1 gramme of hydrogen together with 7·98 grammes of oxygen from 0° to 18°. Since the specific heats for equal weights under constant pressure of oxygen and hydrogen are respectively 0·218 and 3·405, the value of $a-b$ is sensibly 69 gramme-degrees; so that Thomsen's value becomes 34,207 if Regnault's formula is used, and 34,308 if Bosscha's be employed.

28. Favre and Silbermann found (*Annales de Chimie* [3], xxxiv. p. 357) that when 9 grammes of water were formed from oxygen and hydrogen, the gases being at a mean temperature of 9° (at 6° to commence with and 12° to end with), and the water produced being at 6° (the heat required to raise the temperature of the water produced being taken into account in determining the water-equivalent of the calorimeter), the average heat-evolution sufficed to raise 34,462 grammes of water 1°, the water being at the average temperature of 9°. According to Regnault's formula the specific heat of water at 9° is 1·00043, and according to Bosscha's 1·00198; whence the heat-evolution per 8·98 grammes becomes

$$\frac{8\cdot98}{9}\times34462\times1\cdot00043=34400,$$

or

$$\frac{8\cdot98}{9}\times33462\times1\cdot00198=34453,$$

according as the one or the other formula is used. To this must be added a quantity $c-d$ to reduce the value to that which would have been produced had the gases and water produced been all at 0°: c is the heat given out by 8·98 grammes of water in cooling from 6° to 0°, whilst d is the heat required to raise the hydrogen and oxygen from 0° to 9°; whence $c-d$ is sensibly 8 gramme-degrees. So that the heat-evolution becomes 34,408 by Regnault's formula, and 34,461 by Bosscha's formula.

29. Andrews found (Phil. Mag. 1848, vol. xxxii. p. 321) that a litre of dry hydrogen gas at 0° and 760 millims., in combining with oxygen, the gases being at the average temperature of 19°·7, produced (on an average of four accordant experiments) enough heat to raise 3036 grammes of water 1°, the water being at an average temperature of 20°·7, and the calculation not including the heat required to raise the water produced from the average initial temperature of 19°·7 to the average final temperature 21°·8. Since a litre of hydrogen at 0° and 760 millims. weighs 0·089578 gramme (Regnault), the corrected heat-evolution per gramme of hydrogen burnt (*i. e.* per 8·98 grammes of water produced) would suffice to heat $\frac{3036}{0\cdot089578}+19=33{,}911$ grammes of water 1°, the water

being at 20°·7. According to Regnault's formula the specific heat of water at 20°·7 is 1·00122; according to Bosscha's formula, 1·00455. Hence the heat-evolution is 33911 × 1·00122 = 33952 (Regnault's formula), or 33911 × 1·00455 = 34065 (Bosscha's formula). To this must be added a quantity $e-f$ to reduce the value to that which would have been produced had the gases and water formed been all at 0°, e being the heat given out by 8·98 grammes of water in cooling from 19°·7 to 0°, and f that required to heat up the constituent hydrogen and oxygen from 0° to 19°·7; so that $e-f$ is sensibly 76 gramme-degrees; whence finally the heat-evolution becomes 34,028 by Regnault's formula, and 34,141 by Bosscha's formula.

30. These reduced values may therefore be thus contrasted:—

	By Regnault's formula.	By Bosscha's formula.
Schuller and Wartha . .	34297	34501
Von Than	33928	33928
Julius Thomsen	34207	34308
Favre and Silbermann . .	34408	34461
Andrews	34028	34141
Mean . . .	34174	34268

That is, the average of these different determinations indicates that by the combustion of 1 gramme of hydrogen and 7·98 grammes of oxygen to water, the gases and water produced being at 0° and 760 millims. throughout, the heat produced would raise the temperature of 34,174 or 34,268 grammes of water from 0° to 1° C., according as Regnault's or Bosscha's formula for the specific heat of water is used. It is noticeable that the latter formula makes Schuller and Wartha's most carefully made experiments appear abnormal, by showing a much greater divergence, both from von Than's (made by essentially the same method) and from the general average, than is the case when Regnault's formula is employed; and hence an additional reason is derived for viewing Bosscha's alleged corrections of Regnault's numbers as being more open to objection than these numbers themselves.

If the combination of the oxygen and hydrogen be supposed to take place at 20° instead of 0° (*i. e.* if the gases used and water produced be all at 20°), the heat-evolution will be less than that calculated above by $g-h$ gramme-degrees, where g is the heat required to raise 8·98 grammes of water from 0° to 20°, and h that requisite to raise the constituent gases through the same range, *i. e.* where $g-h=$ sensibly 77 gramme-degrees. Whence, if water be decomposed at 20° and 760 millims. into oxygen and hydrogen at the same temperature and pressure, the heat-absorption is 34,097, using Regnault's formula, and 34,191 using Bosscha's formula. Similarly at 15°, the correction $g-h$ would be 58 gramme-degrees, giving the values 34,116 (Regnault's formula), and 34,210 (Bosscha's formula).

31. On the other hand, experiments have been cited above (§§ 2 & 5), made by Joule, Kiechl, Raoult, and Favre, giving the following values for the heat-absorption during the electrolysis of water at the ordinary temperature:—

Joule	33557
Kiechl	33653
Raoult	34028
Favre	34204
Mean . .	33860

As these values are mostly calculated per 9 grammes of water, this average value should be reduced in about the proportion of $\frac{8\cdot98}{9}$; but since the increase of temperature of the calorimeter observed represents a slightly higher number of gramme-degrees than that stated, on account of the increase of the specific heat of water with the temperature, the average value requires slightly raising from this cause. After making all probable allowances for these corrections, however, the mean value still remains perceptibly lower than that calculated as above from the results of Schuller and Wartha, von Than, Julius Thomsen, Andrews, Favre, and Silbermann.

On the whole, the data at present extant may be taken to show that *when* 8·98 *grammes of water are formed from gaseous oxygen and hydrogen at the ordinary temperature* (15° to 20°) *throughout, the evolution of heat is sensibly that required to raise the temperature of* 34,100 *grammes of water from* 0° *to* 1° C; or when 1 gramme is formed, the heat-evolution suffices to raise 3797 (or practically 3800) grammes from 0° to 1° C., this value being taken from the reductions by Regnault's formula as being probably more accurate than those made by Bosscha's alleged corrected formula.

32. A number of observations, the details of which are given in Part II., show that, admitting the accuracy of Latimer Clark's valuation of the E.M.F. of his standard cell (Proc. Roy. Soc. 1872, vol. xx. p. 444) as being $1{\cdot}457 \times 10^8$ C.G.S. units at 15°, the value of e in the expression $e = \mathrm{E} - \mathrm{H}\chi\mathrm{J}$ (§ 11) is $1{\cdot}5003 \times 10^8$ C.G.S. units when water is electrolyzed at a temperature of 15°–20°, the value of J being taken as 42×10^6—the probable error of the mean result being $\pm 0{\cdot}0048$, or $\pm 0{\cdot}32$ per cent. Owing, however, to certain causes detailed in Part II. (§ 39), this value is slightly too low—this result being esentially due to the impracticability of collecting every trace of gas evolved, so that the value of H (the heat evolved per gramme-equivalent of water decomposed) is slightly overvalued, and hence e slightly undervalued.

From the combustion experiments of previous investigators, summarized in the preceding eight paragraphs, it results that the value of e in the expression $e = \mathrm{H}'\chi\mathrm{J}$ (§ 11) is $1{\cdot}5038 \times 10^8$ (since $\mathrm{H}' = 34{,}100$), J being, as before, taken as 42×10^6. These numbers, $1{\cdot}5003 \times 10^8$ and $1{\cdot}5038 \times 10^8$, accord so closely as to show that (so far as these experiments can decide) the value of J cannot be far from 42×10^6. Applying the formula

$$\mathrm{J} = \frac{\mathrm{E}}{(\mathrm{H} + \mathrm{H}')\chi}$$

(§ 24) to the experimental data given in Part II., the average value deduced for J is $41{\cdot}96 \times 10^6$, this value for J giving the following values for e:—

From formula $e = \mathrm{E} - \mathrm{H}\chi\mathrm{J}$ . . $e = 1{\cdot}5023 \times 10^8$
" " $e = \mathrm{H}'\chi\mathrm{J}$ . . . $e = 1{\cdot}5024 \times 10^8$.

Since, however, the mean value of H is slightly overvalued, it results that the value of J finally deduced is slightly *above* $41{\cdot}96 \times 10^6$, with a probable error of less than $\pm 0{\cdot}4$ per cent., *i. e.* of less than $\pm 0{\cdot}016 \times 10^6$, due to the electrolytic experiments, the probable error in the determination of H′ not being taken into account.

33. It is evident that, considering the nature of the data from which this conclusion is drawn, not much dependence

can be placed on the final value of J deduced except as an approximation. It is noteworthy, however, that this value of J very closely agrees with that deduced by Joule in 1867 B. A. Reports, 1867), by determining the heat producible in a wire of known resistance by a known current, and that both are perceptibly higher than the value obtained by Joule by means of water-friction &c. in 1850 (Phil. Trans. 1850, p. 61); whilst this older value is completely corroborated by his recently made experiments on water-friction &c. (Phil. Trans. 1878, p. 365). In 1850 Joule found that the heat required to raise 1 lb. of water (weighed *in vacuo*) from t to $t+1$ (where t lies between 55° and 60° F.) corresponded to the following number of foot-pounds at Manchester:—

By friction of water . . .	772·692
" " mercury . .	774·083
" " cast iron . .	774·987

Of these values the first was considered probably the most accurate; and as a little vibration and sound was unavoidable in the experiments, the round number 772 was adopted as the most probable value. The 1878 value finally arrived at is that the heat required to raise 1 lb. of water (weighed *in vacuo*) from 60° to 61° F. corresponds to 772·55 foot-pounds, and is hence sensibly identical with the former result. The 1867 values, however, were as follows, expressed in foot-grain-second units:—

		Per grain degree F. (water weighed in air).
1st series.	Mean of 10 experiments . .	25335
2nd "	" 15 " . .	25366
3rd "	" 30 " . .	25217

Of these the last value was taken as most exact; when reduced to water weighed *in vacuo* it becomes 25187. As in this series the average temperature of the calorimeter was 18°·63 C. = 65°·5 F., the final result is that, to raise the temperature of 1 gramme of water from 65° to 66° F., requires 25,187 foot-grain-second units of work.

34. When reduced to C.G.S. measure and degrees Centigrade the three values, 772·692 foot-pounds, 772·55 foot-pounds, and 25,187 foot-grain-second work units, become respectively 41·60, 41·59, and 42·14 megalergs per gramme-

degree, the value of g at Manchester being taken at 981·3. (Compare illustrations of the C.G.S. system of units, by Prof. Everett, p. 51.) Since, however, the specific heat of the water at the three assigned temperature-limits, 55°–60°, 60°–61°, and 65°–66° F., is not quite the same, on reduction to the gramme-degree taken above as unit (the heat requisite to raise 1 gramme of water from 0° to 1° C.), these values become

$$\frac{41{\cdot}60}{1{\cdot}0008}=41{\cdot}57,\quad \frac{41{\cdot}59}{1{\cdot}0009}=41{\cdot}54, \text{ and } \frac{42{\cdot}14}{1{\cdot}0010}=42{\cdot}10$$

megalergs respectively when Regnault's formula for the specific heat of water (§ 25) is taken; whence the mean value of J is 41·555 megalergs from the two sets of friction experiments (1850 and 1878), and 42·10 by the electric-current method (1867)—the latter consequently being $\frac{42{\cdot}10}{41{\cdot}555}=$ 1·0131 times the former, or 1·31 per cent. in excess. The value for J deduced above from the writer's experiments (viz. 41·96 megalergs) is $\frac{41{\cdot}96}{41{\cdot}555}=1{\cdot}0098$ times that got from Joule's friction experiments, showing an excess of 0·98 per cent. Hence, on the whole, the writer's numbers and Joule's 1867 values concord much more exactly than might have been anticipated, each showing an excess over Joule's water-friction value (1850 and 1878) of about 1 per cent., Regnault's formula being employed for reducing the observed rise of temperature to gramme-degrees at 0°–1° C.

35. Reasons have already been given (§ 32, *vide* also Part II. § 39) for supposing that the value 1·5003 found for the E.M.F. representing the affinity for oxygen and hydrogen in water is slightly understated. The effect of an error of this kind would be to raise the value of J deduced by the formula

$$J=\frac{E}{(H+h)\chi} \quad (\S\ 24),$$

and hence to make the excess of the value of J thus deduced over Joule's water-friction value to be somewhat more than the 1 per cent. or thereabouts just calculated. It is worthy of notice that there is also reason for supposing that Joule's 1867 value is understated. Experiments are described in Part II. the result of which is to show that an unobserved source of error existed in Joule's 1867 experiments, the effect of which was to diminish the value of J thence deduced by an amount probably equal to some tenths per cent.; so that on the whole Joule's 1867 value, when corrected, is probably from 1·5 to 2·0 per cent. higher than his water-friction value (1850 and 1878).

This difference between the two values is precisely that which would subsist did an error to an equal amount exist in the B.A. resistance-unit valuation: *i. e.* if the B.A. unit were 1·015 to 1·020 earth-quadrant per second instead of being exactly 1 earth-quadrant per second, the value of J deduced from Joule's 1867 experiments would be 1·015 to 1·020 times the true value; for it is calculated by the formula

$$J = \frac{C^2Rt}{H},$$

where C is the current, R the resistance, t the time, and H the heat evolved. Moreover the existence of such an error in the B.A. unit would cause a precisely corresponding error in Latimer Clark's valuation of the E.M.F. of his standard cell (Proc. Roy. Soc. 1872, vol. xx. p. 444), this valuation being based on the determination of the resistance R between two points traversed by a known current C, the potential difference E between the points being equal to the E.M.F. of the cell, so that by Ohm's law $E = CR$; and the effect of this would be to cause a precisely similar error in the value of J deduced by the formula $J = \frac{E}{(H+h)\chi}$, as in the writer's experiments.

36. In confirmation of the supposition that the B.A. unit of resistance is too large, Kohlrausch's experiments may be cited (Phil. Mag. 1874, vol. xlvii. pp. 294 & 342). A careful comparison of several copies of the B.A. unit with coils standardized by himself in absolute measure led Kohlrausch to the conclusion that the B.A. unit is really 1·0196 earth-quadrant per second, or 1·96 per cent. too large; for he found that the resistance of certain coils graduated in Siemens mercury units averaged $0{\cdot}9717 \times 10^9$ C.G.S. units, whilst the B.A.-unit coils averaged 1·0493 times the resistance of the Siemens coils, giving therefore for the B.A. unit the value

$$0{\cdot}9717 \times 1{\cdot}0493 \times 10^9 = 1{\cdot}0196 \times 10^9.$$

On the other hand, it is to be observed that Lorenz has made a determination of the value of a Siemens mercury unit with a lower result (Pogg. *Ann.* 1873, vol. cxlix. p. 251), viz. 0·9337 earth-quadrant per second. Lorenz's mercury-tubes were made by himself; and it does not seem from his paper that any evidence was obtained of the purity of the mercury used other than the fact that its specific gravity at 5°·3 C. was 13·586 after digesting for a long time with nitric acid. These mercury-tubes do not seem to have been compared directly with any B.A.-unit coil so as to give the value of the B.A. unit in absolute measure; on comparing them with a reputed Siemens unit, Lorenz found a difference of nearly 1 per cent. ($0{\cdot}943 \times 10^9$ as against $0{\cdot}9337 \times 10^9$)—the difference between Lorenz's mercury-tube value of a Siemens unit and the value of the latter in B.A. units, as deduced by Kohlrausch, being nearly exactly double this amount, or nearly 2 per cent., $\frac{1}{1{\cdot}0498} \times 10^9 = 0{\cdot}9530 \times 10^9$ as against $0{\cdot}9337 \times 10^9$.

37. Yet, again, values for J have been obtained by other observers considerably higher than that deduced from Joule's water-friction experiments. The following Table (mainly taken from Prof. G. C. Foster's article "Heat" in Watts's 'Dictionary of Chemistry,' 1st Supplement, p. 687) gives the chief of these results expressed in gravitation measure, no corrections being made for differences in the value of g at the various localities where the experiments were made, or in the specific heat of water at different temperatures. The last column gives the approximate ratio of the value found to that deduced by Joule from water friction, this latter being taken as 423·5 metre-grammes per gramme-degree Centigrade:—

Observer.	Method employed.	Value of J found.	Ratio to Joule's water-friction value.
Hirn	Crushing of lead	425·2	1·004
„	Friction of water and brass..........................	432	1·020
„	Escape of water under pressure	433	1·023
„	Specific heats of air	441·6	1·043
Joule	Heat produced by electric currents	429·3	1·014
Violle......	„ „ induced currents (aluminium)	434·9	1·027
„	„ „ „ „ „ (copper) ...	435·2	1·028
„	„ „ „ „ „ (tin)	435·8	1·029
„	„ „ „ „ „ (lead)	437·4	1·033
Regnault...	Velocity of sound	437	1·032

On the whole, it is evident that further experiments are desirable as to the development of heat in circuits of resistance measured in terms of the B.A. unit by known currents, so as to establish as accurately as possible a connexion between the values of J and of the B.A. unit, so that if the first is assumed to be correctly given by Joule's water-friction value the error (if any) in the B.A. unit can be calculated, or so that the value of J can be determined afresh when the precise value of the B.A. unit is known from further experiments. The writer hopes shortly to be enabled to communicate the results of experiments in this direction made by a method differing from that employed by Joule in certain important points, so as to avoid the sources of error existent in Joule's 1867 experiments (Part II.).

PART II.—*By* Dr. C. R. ALDER WRIGHT *and* E. H. RENNIE, *M.A.* (*Syd.*), *B.Sc.* (*Lond.*), *Demonstrator of Chemistry in St. Mary's Hospital Medical School.*

Experimental Determination of the Electromotive Force corresponding to the Work done in the Decomposition of Water into Oxygen and Hydrogen at the ordinary temperature.

38. In order to apply the principles described in Part I. (§§ 11, 18, 32), the current from a Daniell battery was passed through a voltameter placed in a calorimeter, and the average difference of potential between the voltameter-plates determined by connecting them with a quadrant electrometer standardized by a Clark cell (verified for us by Dr. Alexander Muirhead). The amount of decomposition being determined ($=n$ grammes), and the quantity of heat, h, developed in the voltameter observed, the data were obtained for the calculation of the E.M.F. representing the nett work corresponding to the sum of the physical and chemical changes taking place by the formula

$$e=\mathrm{E}-\frac{ha\chi\mathrm{J}}{n},$$

a being the equivalent of the electrolyte.

In the case of water acidulated with sulphuric acid, the following results were obtained. The voltameter consisted of a wide test-tube of about 30 millimetres diameter, into the neck of which was fixed an india-rubber cork perforated with three holes: through the centre one passed a delivery tube of small bore for the collection of the evolved gases; through the other two, copper rods 5–6 millims. in diameter. To the lower ends of these were soldered equally thick platinum rods, the free ends of which were previously forged into spade-like plates which were arranged parallel to one another; the solderings and the portions of the copper rods inside the test-tube were imbedded in a thick mass of gutta-percha, with the threefold object of keeping the plates at an invariable distance from one another (the voltameter being also intended for some other experiments in which this was essential), of protecting the copper and soldering from corrosion by the acid and the consequent introduction of metals into the solution electrolyzed, and of filling up the upper space in the tube, so that any error due to alteration in temperature of the voltameter and consequent retention therein of varying quantities of gas should be rendered negligible. The projecting ends of the rods were passed through small india-rubber corks and amalgamated; by then fixing short pieces of wide glass tubing over these smaller corks and pouring mercury into the cups thus formed, connexion by mercury-cups could readily be established between the battery and the voltameter-plates.

This arrangement of mercury-cups was also applied to the copper Daniell-battery cells, a thick wire bent downwards and amalgamated at the end being also soldered to each zinc plate so as to dip into the mercury-cup of the next cell. Much trouble in brightening connexions &c. was thus saved; the current could readily be broken instantaneously by simply lifting one of the zinc plates an inch or so, so that the wire no longer dipped into the mercury; whilst an easy means was afforded of introducing more cells or shutting some out from the circuit when required without actually interrupting the current.

39. A number of attempts were made to utilize Bunsen's ice-calorimeter for the measurement of the heat evolved; the construction of apparatus sufficiently large to enable considerable amounts of heat to be measured accurately, however, was found to present some difficulties; whilst with smaller apparatus the errors of measurement of the amount of gas

evolved became considerable, even when the voltameter was connected with the gas-measuring apparatus used for Frankland and Armstrong's water-analysis process; for the occlusion of the evolved gases by the electrodes, the absorption of oxygen by the acidulated water of the voltameter, the production of ozone or of hydrogen dioxide, and the removal of hydrogen whilst still nascent from the one electrode by combination with dissolved oxygen, and of oxygen from the other by combination with dissolved hydrogen, present sources of diminution in the amount of gas actually evolved, which mostly become relatively greater with weak currents, such as would have to be employed with a small ice-calorimeter. Although it did not appear to be impracticable to overcome these difficulties, yet it was found that to do so would require a considerable amount of time and trouble; and therefore we reverted to the use of an ordinary water-calorimeter, employing currents of sufficient magnitude to furnish upwards of 500 cub. centims. of mixed gases during the time which the experiment lasted (from ten to forty-five minutes, according to the battery power used). Even with the strongest currents used, representing about 0·65 C.G.S. current-unit, the amount of substances formed capable of liberating iodine from potassium iodide corresponded to no more than ·001 to ·002 gramme of iodine per 500 cub. centims. of gas produced, representing a diminution in the volume of gas evolved practically quite inappreciable. The gas-measuring apparatus in our possession being incapable of measuring so large a quantity as 500 cub. centims., we employed the method used by Joule for determining the volume of gas produced, viz. collection over water in a bottle, the weight of which was known when filled with water and closed by a stopper. To determine the gas collected, the bottle was immersed for some minutes in a large bucket of water, the temperature of which was known; when the gas had attained the temperature of the water, the bottle was raised until the level of the fluid inside and out was the same (*i. e.* until the pressure was atmospheric); the stopper was then inserted and the bottle removed, wiped dry, and weighed. In this way a close approximation to the quantity of gas produced was obtained, the error being one of defect, owing to the sources above mentioned and the absorption of oxygen by the water during the process of collection. From the number of cub. centims. of mixed gases thus obtained (reduced to dryness, 0° C., and 760 millims.) $=v$, the weight of water decomposed, w, was calculated by the formula

$$w=v\times 0{\cdot}0005363,$$

the coefficient 0·0005363 being deduced from Regnault's observations that 1 litre of dry hydrogen and oxygen at 0° and 760 millims. weigh respectively 0·089578 and 1·429802 gramme,

whence 2 grammes of hydrogen occupy 22·327 litres,
and 15·96 „ oxygen „ 11·162 „
so that 17·96 grammes of detonating gas occupy 33·489 „

or 1 cub. centim. of detonating gas weighs $\frac{17{\cdot}96}{33489}=0{\cdot}0005363$ gramme.

40. The calorimeter employed in these experiments consisted of a glass beaker capable of holding about 1500 cub. centims., fitted closely inside a polished tin cylinder supported (on a wooden block and several folds of wadding) concentrically inside a similar tin cylinder, so that about an inch of air-space everywhere intervened between the two tin surfaces, the two tin cylinders being kept in the same relative positions by cork wedges. This outer cylinder was weighted with lead and sunk inside a third similar larger cylinder filled with water; so that the calorimeter itself was surrounded on all sides, saving the top, by a water jacket. Polished tin lids for the calorimeter and the outermost vessel were provided, perforated with holes for the passage of the voltameter (in the axis of the cylinders), of the thermometers employed, and of the stirrers; these latter consisted of annular horizontal metallic plates, with stout vertical wires attached, so that by moving them up and down a very effective agitation could be accomplished. The thermometers were graduated in millimetres, and were carefully calibrated and checked against one another and against a Kew-standardized thermometer. The one used for the inner vessel was found to be sensibly uniform in calibre at the part where the observations were made (chiefly that corresponding to 12°–20° C.); and at this part the value of 1° C. was 11·28

millims. Several careful determinations of the water-equivalent of the calorimeter (including the stirrer, thermometer-bulb, and voltameter, which always contained 20 grammes of acidulated water containing 22 per cent. of H_2SO_4,) gave numbers varying from 1092 to 1097, and averaging 1094·5 grammes when 1000 grammes of water (weighed in a vacuum) were placed therein.

41. In order to obtain with as great a degree of accuracy as possible the corrections for radiation with this instrument, a large number of observations were made of the rate of alteration of the reading of the calorimeter-thermometer under various conditions, viz. when the water jacket was hotter or colder than the calorimeter, and when the calorimeter was warmer or colder than a thermometer-bulb placed a few inches above the outer lid, so as to indicate the temperature of the air together with the effect produced by radiation from the hand during stirring. It was found that all the observations could be expressed with a fair degree of accuracy by the formula

$$x = 0{\cdot}16\,(M - m) + 0{\cdot}0225N,$$

where x is the alteration of the calorimeter-temperature (*loss* of heat) in millimetres per hour, M the difference between the average readings of the calorimeter and water-jacket thermometers during the period of observation, m a small correction (derived from a specially constructed table) to reduce the scale-reading of the second of these thermometers to that of the first, and N the difference between the average temperature of the calorimeter and that of the air, as indicated by the third outer thermometer and expressed in terms of the scale of the calorimeter-thermometer. Thus, for example, the following table illustrates the observed and calculated values of x. It is noticeable, *en passant*, that whilst the heating or cooling effect of the air on the calorimeter exerted through the double lid is, for equal differences of temperature, much less than that of the water jacket, it is still not negligible, amounting to about $\frac{1}{7}$ of that of the jacket.

Water jacket warmer than room. Calorimeter colder than jacket and warmer than room.		Water jacket colder than room. Calorimeter colder than jacket and colder than room.		Water jacket colder than room. Calorimeter slightly warmer than jacket and colder than room.		Water jacket colder than room. Calorimeter warmer than jacket and slightly colder than room.	
Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
−4·8	−4·9	−8·0	−8·2	+0·2	+0.1	+2·3	+2·3
−4·0	−4·1	−7·4	−7·3	−0·2	−0·2	+1·9	+1·9
−3·6	−3·6	−7·0	−6·7	−0·7	−0·6	+1·2	+1·4
−3·2	−2·7	−5·1	−5·4	−1·0	−1·0	+1·0	+1·3
−2·4	−2·0	−4·8	−4·8	−1·5	−1·2	+0·8	+0·8
−1·5	−1·3	−4·3	−4·3	−1·4	−1·2	+0·3	+0·3
−0·9	−0·7	−3·8	−3·9	−1·0	−1·1	+0·2	0
−0·1	0	−3·6	−3·5				
+0·9	+0·7						
+1·2	+1·3						
+1·5	+1·8						
+1·8	+2·1						

42. It is evident from these numbers that the difference between the observed and calculated rates of alteration per hour rarely exceeds 0·2 millimetre, and averages much less; so that the difference between the results deduced by the formula and those attained by direct observation is practically negligible for all experiments lasting for only half an hour or less. In order, however, to diminish any error in the radiation-correction caused by the use of the formula, a threefold determination was made, as follows, the average of the three observations being taken. First, the correction was estimated from the observed average values of M and N throughout the experiment. Secondly, the rate of alteration of temperature of the calorimeter (rising) before the experiment was commenced was determined by observations made for 30–60 minutes before commencing; and, similarly, the rate of alteration (falling) was determined by observations made for 30–60 minutes after concluding, and the mean of the two rates taken. Thirdly, the rate of alteration at the commencement of the experiment was calculated from the values of M and N then observed; and similarly the rate of alteration at the close was calculated from the then values of M and N, and the mean taken as before. The three values rarely differed from their

mean by so much as 0·1 millimetre per hour. Thus the following values were obtained in an experiment lasting eighteen minutes, the + value meaning loss of heat, and the − sign gain*.

			Millims. per hour.
Value of x obtained by first method (formula applied to mean values of M and N)			−1·00
Value of x obtained by second method.			
Observed rate of alteration before experiment commenced	−4·1	Mean	= −1·05
Observed rate of alteration after experiment concluded	+2·0		
Value of x obtained by third method.			
Calculated rate of alteration at commencement	−4·0	Mean	= −1·02
Ditto at close	+1·95		
		Average	−1·02

Hence the radiation-correction for the eighteen minutes during which the experiment lasted is $-\frac{18}{60} \times 1{\cdot}02 = -0{\cdot}31$.

43. The above correction, however, is not the only one to be applied; for the water in the voltameter gets heated more quickly than the calorimeter, and after the end of the experiment some time is required for the heat retained in the voltameter to become uniformly diffused throughout the calorimeter by passage through the glass wall of the voltameter. It was found that 6 or 7 minutes sufficed to bring about perfect equalization of temperature; so that during the first 5 minutes or so the temperature of the calorimeter slightly rose, and then began to fall again from the cooling effect of the radiation. Accordingly, to determine the true amount of rise in temperature during the experiment, the thermometer was read 8, 10, and 12 minutes after the conclusion—the loss by radiation during these several periods of 8, 10, and 12 minutes

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* This particular experiment is selected as an illustration because the temperature-corrections detailed in this and the subsequent paragraphs are *larger* than those in almost any other of the 18 experiments made in all (§ 46).

being added on to the observed temperature at the ends of these periods severally; the mean of the three numbers thus obtained was taken as the true temperature that would have been observed at the end of the experiment had the heat retained in the voltameter been diffused throughout the calorimeter at that moment. The loss during these three periods is calculated from the data in the latter two methods just described for the determination of the radiation-correction during the experiment: thus, in the illustration given,

	Millims. per hour.
Observed rate of alteration after experiment concluded	+2·0
Calculated ” ” ”	+1·95
Mean	+1·97

Hence the losses by radiation during 8, 10, and 12 minutes are respectively, +0·26, +0·33, and +0·39; and the true rise in temperature of the calorimeter is found thus:—

	t=8.	*t*=10.	*t*=12.
Reading of calorimeter-thermometer *t* minutes after conclusion of experiment	201·05	201·0	200·9
Radiation-loss during *t* minutes	+0·26	+0·33	+0·39
Calculated temperature of calorimeter at close of experiment	201·31	201·33	201·29

Average	201·31	millims.
Reading of calorimeter-thermometer at commencement of experiment . . .	166·7	”
Calculated rise of thermometer	34·61	”
Correction for radiation during experiment	− 0·31	”
Corrected rise of thermometer	34·30	”

Since the water-equivalent of the calorimeter &c. is 1094·5, and 11·28 millims. of the thermometer-scale represent 1° C., the amount of heat actually produced is

$$\frac{34{\cdot}30}{11{\cdot}28} \times 1094{\cdot}5 = 3328 \text{ gramme-degrees.}$$

44. Two other small corrections also have to be applied to the heat-evolution thus determined. In the first place, the escaping gases pass out saturated with moisture, the evaporation of which absorbs a certain amount of heat. In each experiment the average temperature was close to 15°, at which temperature the tension of aqueous vapour is 1·27 centim. or $\frac{1\cdot27}{76}$ atmosphere, $=1\cdot67$ per cent. Hence every 1000 cub. centims. of evolved gas would contain 16·7 cub. centims. of water-vapour (at 0° and 760), weighing 0·0134 gramme; the latent heat of water-vapour at 15° being nearly 600, this would represent very nearly 8 gramme-degrees per litre of evolved gases. Inasmuch as the gases were evolved not from pure water but from water containing 22 per cent. of sulphuric acid, it is likely that the gases actually evolved contained slightly less moisture than the normal saturating amount; but as in most of the experiments only about 500 to 550 cub. centims. of gases were evolved, the total correction is only about 4 gramme-degrees, and a trifling error in its estimation is negligible.

In the next place a minute amount of heat is absorbed in separating from sulphuric acid the water electrolyzed. From the experiments of Favre and Quaillard (*Comptes Rendus*, l. p. 1150), 98 grammes of sulphuric acid already mixed with 216 of water (forming a dilute acid of composition $H_2SO_4+12H_2O$) evolve 483 gramme-degrees when 72 grammes more of water are added (forming $H_2SO_4+16H_2O$). The further addition of 72 grammes more of water evolves 222 gramme-degrees, whilst the addition of a third 72 grammes (forming $H_2SO_4+24H_2O$) evolves 141 gramme-degrees. From these numbers, by interpolation, it results that acid of strength $H_2SO_4+20H_2O$ (containing 22 per cent. of H_2SO_4) will evolve close upon 2·5 gramme-degrees when 1 gramme of water is added to such a quantity of acid as not sensibly to alter its composition by dilution; *i. e.* that for every gramme of water decomposed by electrolysis 2·5 gramme-degrees are absorbed.

In the experiment the calorimetric details of which have just been given, the corrected heat-evolution finally becomes as follows:—

	gramme-degrees.
Heat-evolution calculated from rise of thermometer corrected for radiation &c. . .	3328
Correction for heat absorbed by moisture in 0·4967 gramme of mixed oxygen and hydrogen evolved during experiment . . .	9·2
Correction for heat absorbed in separating 0·4967 gramme of water from dilute sulphuric acid	1·2
	3338·4

45. The value of E, the average difference of potential between the voltameter plates, was found thus. Before the experiment commenced the scale of the electrometer was standardized by means of a Clarke cell, several readings being taken by reversal so as to obtain total readings each equal to double the deflection; after the experiment was concluded the scale was again similarly standardized. The difference between the two standardizings was always very small or nil, but frequently was just perceptible, owing to leakage of electrometer charge; the mean of the two standardizings was taken to represent a potential difference of

$$1{\cdot}457\,\{1-(t-15)\times 0{\cdot}0006\}\ \text{volt},$$

where t is the Centigrade temperature, and 0·0006 the alteration in E.M.F. per 1° C. (equal to 0·06 per cent. per 1°). As in the whole series of experiments taken together the average temperature of the cell was very close to 15°, the cell was assumed to have the constant average value 1·457 volt throughout. Throughout the experiment the potential difference between the electrodes was determined at as nearly as possible equal intervals of time by reversal, the quadrants being connected with the mercury-cups of the voltameter through the reversing-gear. As a general rule, the indicated potential-difference rose steadily throughout the experiment to the extent of from 3 to 6 per cent. of its initial value (owing to polarization of the electrodes); but in some cases this effect was just about compensated by a gradual diminution in current-strength, so that the potential difference remained nearly the same or slightly fell[*]. In the experiment the calorimetric details of which have just been given, the following values were obtained:—

	Scale-divisions.
Value of Clark cell before experiment . .	80·65
" " after " . .	80·55
Mean	80·60

Average of 23 pairs of readings of potential difference between voltameter-plates (commencing with 237·5 and slowly sinking to 231·0)=232·40.

Average potential-difference in volts,

$$\frac{232{\cdot}40}{80{\cdot}60} \times 1{\cdot}457 = 4{\cdot}201.$$

Hence, taking $a=8{\cdot}98$, $\chi=0{\cdot}000105$, and $J=42\times10^6$, the value of e in the expression

$$e=\mathrm{E}-\frac{ha\chi\mathrm{J}}{n}$$

becomes $1{\cdot}540\times10^8$, since $\mathrm{E}=4{\cdot}201\times10^8$, $h=3338$, and $n=0{\cdot}4967$.

46. In precisely the same way the following numbers were obtained in seventeen other experiments.

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[*] The fluctuations in the current and in the potential difference set up between the electrodes necessarily cause corresponding fluctuations in the amount of decomposition per second and the quantity of heat developed per second, and hence are allowed for, and (if not too great in extent) produce no effect at all on the end result. In just the same way, it is immaterial whether the electrodes are perfectly parallel and equal in size, whether the current passes more between their edges than between their centres, of what material they are made, and so on; for what is actually done is to measure the difference of potential between the mercury-cups at the ends of the thick copper rods connected with the electrodes, and also the quantity of electricity passing, and hence the total work done between the cups. The resistance of the cups and thick copper rods being inappreciable, all this work must be done inside the voltameter between the electrodes, either as heat or chemical decomposition; the former being measured directly, the amount of the work representing the latter is known by difference.

Battery-power employed.	Time in minutes.	E. Average difference of potential between electrodes in volts.	n. Grammes of water decomposed.	h. Heat evolved		Value of $\frac{h}{n}a\chi J$	Value of $E-\frac{h}{n}a\chi J$.
				Corrected for radiation only.	Further corrected for the other two sources of heat-absorption. (§ 44)		
4 Daniell cells.	44	2·995	·2821	1091	1096	$1·539\times10^8$	$1·456\times10^8$
4 ,,	41	3·065	·2740	1094	1099	1·589 ,,	1·476 ,,
4 ,,	34	3·134	·2764	1126	1131	1·621 ,,	1·513 ,,
5 ,,	24	3·336	·2687	1122	1227	1·808 ,,	1·528 ,,
6 ,,	23	3·380	·2695	1289	1294	1·901 ,,	1·479 ,,
6 ,,	22	3·369	·2676	1262	1267	1·875 ,,	1·494 ,,
8 ,,	16	3·714	·2785	1543	1548	2·200 ,,	1·514 ,,
8 ,,	15	3·685	·2794	1567	1572	2·228 ,,	1·457 ,,
8 ,,	14	3·746	·2765	1552	1557	2·230 ,,	1·516 ,,
10 ,,	14	3·777	·2708	1553	1558	2·278 ,,	1·499 ,,
10 ,,	13·5	3·845	·2815	1659	1664	2·341 ,,	1·504 ,,
12 ,,	13	3·805	·2708	1534	1539	2·250 ,,	1·555 ,,
12 ,,	13	3·830	·2729	1606	1611	2·337 ,,	1·493 ,,
12 ,,	12	3·979	·2877	1833	1838	2·530 ,,	1·449 ,,
12 Daniells and 6 Groves in weak action.	18	4·201	·4967	3328	3338	2·661 ,,	1·540 ,,
Ditto	17	4·242	·4934	3368	3378	2·711 ,,	1·531 ,,
12 Daniells and 6 Groves in full action	9·25	4·393	·3379	2479	2484	2·912 ,,	1·481 ,,
Ditto	8	4·442	·2853	2100	2105	2·921 ,,	1·521 ,,
						Average ...	$1·5003\times10^8$

Maximum deviation from mean.

On side of excess . $\dfrac{1·555-1·5003}{1·5003}=3·65$ per cent.

On side of deficiency $\dfrac{1·5003-1·449}{1·5003}=3·42$,,

per cent.
"Probable error" of one determination $\pm0·0202=\pm1·34$
,, ,, result . . . $\pm0·0048=\pm0·32$

Experiments on a possible Source of Error in Joule's *Valuation of* J *made in* 1867 (Brit. Assoc. Reports).

47. The method used by Joule in these experiments consisted essentially in the comparison of the resistances of a platinum-silver wire (made into a coil and immersed in a calorimeter) and of copies of the B.A. unit, and then passing a current, measured by a tangent-galvanometer, through the

coil and determining the amount of heat produced therein in a given time. The mean temperature of the calorimeter throughout a series of observations being determined, the resistance which the wire would possess at that temperature was calculated from the observed resistance at some other temperature, the rate of variation in resistance of the wire with temperature used being known. Calling this calculated resistance R, the average current being C, and the time t seconds, the value of J was deduced by the formula $J=\frac{C^2Rt}{H}$, where H is the heat evolved.

It is evident that any error in the valuation of R must produce an error to exactly the same extent in the value of J thus deduced; and that such a source of error must have existed is evident from the circumstance that inasmuch as heat was continuously developed in the interior of the wire, and as an interval of time must necessarily elapse before heat could pass from the interior of the wire into the water by conduction, the temperature of the interior of the wire, and consequently the mean temperature of the wire as a whole, must have been somewhat higher than the mean temperature of the calorimeter. Apart from this, too, the wire must have been heated to a small extent above the whole mass of water in the calorimeter, owing to the adhesion to the wire of a film of warmed water which could not possibly be displaced by stirring absolutely *pari passu* with its becoming warmed. The very efficient automatic continuous stirring-arrangement adopted by Joule in the third series of investigations detailed in the paper referred to must necessarily have reduced this source of superheating of the wire to a minimum; but it is doubtful if it would wholly remove the additional tendency to superheating from this cause.

48. In order to form some idea of the possible extent to which the wire might thus become superheated by currents of the magnitude used by Joule*, a platinum wire about 50

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* In Joule's experiments the galvanometer made one turn only of 0·62723 foot radius, the angular deflection varying from 26° to 32°; hence the currents must have lain between $\frac{0{\cdot}62723\times 30{\cdot}48}{2\pi}.\mathrm{I}.\tan 26°$ and $\frac{0{\cdot}62723\times 30{\cdot}48}{2\pi}.\mathrm{I}.\tan 32°$, where 30·48 is the number of centims. in a

centims. long and 1·15 B.A. unit resistance was twisted into a spiral, the ends of which were soldered to thick copper rods surmounted with mercury-cups and passing through an india-rubber cork to keep them together: this spiral was immersed in a beaker containing about a litre of distilled water, the temperature of which was read off to 0°·1 degree by a thermometer placed with its bulb almost touching the centre of the spiral; by means of a broad horizontal ring of metal attached to a stout wire the water could be briskly agitated, to a much greater extent indeed than would be safe with a calorimeter in which splashing must be avoided. Currents of different strengths were then passed through the coil for periods of time varying from 20 minutes to an hour, a voltameter being also included in the circuit, and the total gas evolved collected as previously described. During this time the water was kept continually stirred, and its temperature observed at equal intervals of time, whilst the difference of potential existing between the ends of the wire was observed as frequently as possible by connecting the mercury-cups with a quadrant-electrometer standardized by a Clark cell before and after the experiment, precisely as above described. During the first two or three minutes the electrometer-readings always diminished through diminution of current by polarization of the voltameter-plates; afterwards they remained nearly constant, gradually rising through the increasing mean temperature of the wire, or slightly sinking if the current-strength diminished through continued use of the battery. During the first few minutes the readings were accordingly taken every 10 or 15 seconds without reversal, so as to obtain a large number of observations giving the mean potential difference during this period; subsequently the readings were taken by reversal at the rate of one pair per minute, the readings being divided into a number of groups corresponding to equal intervals of time (3 or 5 minutes), the average readings for each interval being again averaged to give the mean reading for the whole period;

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foot and I the horizontal terrestrial magnetic force. Taking $I=0·18$, the currents must therefore have averaged about 0·3 C.G.S. unit. The resistance of the platinum-silver wire used was nearly equal to 1 B.A. unit $=10^9$ C.G.S. units.

so that for an hour's observation more than 120 total readings were taken.

49. In this way the following numbers were obtained, the amounts of gas collected being translated into C.G.S. current-units by the formula

$$C = \frac{V \times 0{\cdot}0005363}{T \times 60 \times 8{\cdot}98 \times 0{\cdot}000105} = \frac{V}{T} \times 0{\cdot}09479,$$

where C is the C.G.S. current, V the volume of gas (dry, at 0° and 760 millims.), T the time in minutes, and 0·0005363 the weight in grammes of a cubic centim. of mixed gases at 0° and 760 (§ 39); *i. e.* where $\frac{V}{T}$ is the volume of gas evolved per minute. The resistance of the wire is calculated from Ohm's law $\left(C = \frac{E}{R}, \text{ whence } R = \frac{E}{C}\right)$ by dividing the average potential difference by the current. Every experiment was made as nearly as possible in the same way, so as to diminish sources of error and make the results comparable, the only noteworthy difference being that with the stronger currents shorter times were given, and the beaker of water in which the wire was immersed was slightly cooled at intervals during the experiment to avoid the temperature rising too high and having too wide a range.

No. of cells in battery.	Time in minutes.	Gas evolved in cub. cent., dry at 0° and 760 mm.	Average G.C.S. current.	Temperature of water.			Average potential-difference in C.G.S. units.	Resistance in C.G.S. units.
				Initial.	Final.	Average.		
3	60	256·6	0·0406	8·5	10·0	9·24	$0{\cdot}4655 \times 10^8$	$1{\cdot}147 \times 10^9$
3	60	234·5	0·0371	7·5	9·5	8·36	0·4275 „	1·152 „
		Average	0·03885	......	......	8·80	....	1·1495 „
4	30	244·3	0·0772	7·9	9·2	8·50	0·8931 „	1·157
6	30	461·7	0·1459	7·4	9·0	8·20	1·697 „	1·163 „
9	30	777·9	0·2458	7·2	11·0	9·16	2·881 „	1·171 „
9	20	493·4	0·2339	7·4	9·7	8·58	2·732 „	1·168 „
		Average	0·2398	......	......	8·87	....	1·1695 „
12	20	624·7	0·2961	7·1	10·1	8·64	3·469 „	1·172 „
12	20	638·6	0·3027	7·6	10·8	9·18	3·549 „	1·172 „
		Average	0·2994	......	......	8·91	....	1·172 „

Since the average temperature of the water surrounding the wire was nearly the same throughout, not differing by more than a few tenths of a degree from the mean 8°·75, the values in the last column are fairly comparable with one another; whence it is evident that a notable rise in resistance of the wire is produced by each successive increase in current-strength. The resistance of the wire for a current of very small value would clearly lie under $1{\cdot}1495 \times 10^9$; so that the increase in resistance caused by currents of from 0·24 to 0·30 C.G.S. unit is *at least* $\frac{1{\cdot}1695-1{\cdot}1495}{1{\cdot}1495}$ to $\frac{1{\cdot}172-1{\cdot}1495}{1{\cdot}1495}$, or 1·7 to 2·0 per cent.

50. In order to see what increase in mean temperature of the wire over that of the surrounding water this would represent, the fourth experiment was repeated, using warm water (at about 27°) to surround the coil: in this way the following numbers were obtained:—

	C.G.S. current.	Average temperature.	Potential-difference.	Resistance.
Water warmed .	0·1419	27·25°	$1{\cdot}716 \times 10^8$	$1{\cdot}209 \times 10^9$
„ cold . .	0·1459	8·20	$1{\cdot}697 \times 10^8$	$1{\cdot}163 \times 10^9$
Difference .		19·05		$0{\cdot}046 \times 10^9$

Since an increase in resistance of 0·046 corresponds to 19·05 degrees, an increase of 0·0200 to 0·0225 would correspond to 8·3 to 9·3 degrees; *i. e.* currents of 0·24 to 0·30 C.G.S. unit caused a superheating of the wire above the temperature of the surrounding water to an extent averaging 8°·3 to 9°·3*. If it be admitted that in Joule's experiments a similar superheating to the extent of 8° to 9° may have taken place, the effect on the calculated resistance of the platinum-silver used would be to cause the amount to be underestimated by about $8 \times 0{\cdot}031 = 0{\cdot}25$ per cent. to $9 \times 0{\cdot}031 = 0{\cdot}28$ per cent., 0·031 being the percentage increase in resistance per degree of the alloy used by Joule (this alloy was purposely not used by the authors, in order to make the alteration in resistance, if any, more perceptible); that is, the value of J ultimately deduced would be about 0·25 to 0·28 per cent. too low.

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* This increase in resistance with temperature amounts to about 0·21 per cent. per 1° C. Matthiessen found the average increase of pure metals to be about 0·37 per cent. per 1° C., that of an alloy of platinum and iridium containing 33 per cent. of the latter being only 0·06 per cent. per 1°; probably, therefore, this platinum wire was somewhat impure, very likely containing iridium.

51. In order to see whether an increased amount of superheating would be produced by only stirring the water surrounding the wire at intervals of one or two minutes instead of continuously, some of the above observations were repeated under these conditions with the following results, clearly showing that whilst no very material influence is exerted by discontinuous stirring with weak currents, the effect is marked with currents of 0·23 to 0·29 C.G.S. unit.

Cells in battery.	Average temperature.	Average current.	Potential-difference, in volts.	Resistance	
				at average temperature of experiment.	corrected to 8°·75.
3	7·75	0·03155	0·3628	$1·150 \times 10^9$	$1·152 \times 10^9$
6	7·25	0·1350	1·562	1·157 „	1·160 „
9	8·30	0·2285	2·717	1·189 „	1·190 „
12	7·70	0·2880	3·441	1·195 „	1·197 „

52. In order to see whether any analogous effect would be produced by varnishing the wire, as was done in Joule's experiments (owing to possible diminution in rate of passage of heat from wire to water through diminished conducting-power of the varnish), a thin coat of clear filtered shell-lac varnish was lightly applied to the wire; and after two days drying, some of the observations were repeated, with the result of showing that whilst the effect of the varnishing was inappreciable with a weak current, it became noticeable with currents of 0·14 to 0·24 C.G.S. unit. To make sure that the resistance of the wire itself had not been altered permanently by strain &c. during varnishing, the varnish was dissolved off by immersion in alcohol, after which the resistance-value again diminished to its former amount. Thus the following numbers were obtained:—

	Current.	Cells used.	Mean temperature.	Potential-difference.	Resistance.
After varnishing	0·0808	4	° 8·85	0·9347 × 10^8	1·157 × 10^9
Before „ (*suprà*)	0·0772	4	8·50	0·8931 „	1·157 „
			Increase in	resistance ...	Nil.
After varnishing	0·1426	6	8·79	1·675 „	1·174 „
Before „	0·1459	6	8·20	1·697 „	1·163 „
			Increase in	resistance ...	0·011 „
After varnishing	0·2273	9	8·93	2·676 „	1·178 ,
Before „	0·2398	9	8·87	2·806 „	1·169 „
			Increase in	resistance ...	0·009 „
After varnish again dissolved off...... ...	0·2426	9	8·75	2·835 „	1·168 „

Apparently, therefore, with the stronger currents the varnish produced an increase in resistance about equal to that due to an increase in temperature of from 4° to 5°; so that if the varnish in Joule's experiments produced a similar effect, the total superheating must have been near 12° to 14°, representing 0·37 to 0·43 per cent. of error in deficiency in the estimation of R, and consequently of J.

53. Yet another source of error, and in the same direction, in these experiments of Joule's lies in the fact that, in comparing the resistances of the B.A. unit and the experimental wire employed, the method adopted consisted in determining the angular deflections α, β, and γ produced in a tangent-galvanometer on passing the current from a given electromotor (1) through the galvanometer only, (2) through the galvanometer and B.A.-unit coil, and (3) through the galvanometer and experimental wire—the resistance of the experimental wire being called x, and that of the unit coil $=1$,

$$x=\frac{(\tan\alpha-\tan\gamma)\tan\beta}{(\tan\alpha-\tan\beta)\tan\gamma}.$$

Now, were the currents used so feeble that no appreciable amount of heat was developed by them in either wire, or were the two wires compared similarly sized and situated so as to be heated to equal extents, the value of x thus deduced would be subject to no other errors than the instrumental and observational ones; but as the B.A.-unit-coil wires are imbedded in solid paraffin whilst the experimental wire was immersed naked in water (save for the film of varnish), it is evident that, if any heat at all had been generated by the currents employed, the B.A.-unit coil must have been more heated than the experimental wire, as the heat developed could not pass away readily through the badly conducting solid paraffin. The currents actually employed were sufficient to cause deflections of 36° to 37° with a galvanometer of nine turns and 17 inches diameter, and of 34° to 50° with a galvanometer of eighteen turns and 17 inches diameter; so that the currents were of magnitude equal to from

$$\frac{17 \times 2{\cdot}54}{4\pi \times 18} . \text{I} \tan 34 \text{ to } \frac{17 \times 2{\cdot}54}{4\pi \times 9} \times \text{I} \tan 37$$

(where I is the horizontal magnetic terrestrial force $=0{\cdot}18$, and 2·54 is the number of centims. in an inch), or averaged about 0·04 C.G.S. unit. In a wire of 1 B.A. unit resistance a current of this magnitude would produce per minute $\frac{0{\cdot}04 \times 0{\cdot}04 \times 10^9 \times 60}{42 \times 10^6} = 2{\cdot}3$ gramme-degrees, which, assuming the wire to weigh 10 grammes and to have the specific heat 0·04 (approximately that of an alloy of one part silver to two of platinum), would raise its temperature $\frac{2{\cdot}3}{10 \times 0{\cdot}04}$, or nearly 6° per minute.

It would therefore seem from this rough estimate that the heat-development produced in the B.A.-unit coil by the currents used may have been sufficient to raise its temperature perceptibly above that attributed to it; while the same result would not be produced to any thing like the same extent with the experimental wire, owing to the latter being immersed in water instead of solid paraffin. If it be assumed that this heat-development sufficed to raise the average temperature of the B.A.-unit coil during the observations 5° above that of the experimental wire (an amount of heating not at all unlikely to have occurred), the ultimate effect of this would be to cause the underestimation of R, and consequently of J, by $5 \times 0{\cdot}031 = 0{\cdot}15$ per cent.

54. Hence, finally, putting together this source of error and that due to the superheating of the wire (increased by varnish-

ing), there is reason for supposing that the value of J obtained by Joule in 1867 by the electric-current method may be at least 0·5 per cent. too small; that is, instead of giving a value 1·31 per cent. larger than that deduced from Joule's water-friction experiments of 1850 and 1878 (Part I. § 34), the corrected value would probably be at least 1·8 per cent. larger than the water-friction value.

The practical conclusion to be drawn from these experiments, then, is that, in any determination in which it is necessary to pass a current through a wire for any length of time, an appreciable error, through increase in the resistance of the wire, will be brought about by its becoming superheated above the temperature of the medium in which it is placed when the current exceeds a certain limit, the exact value of which necessarily depends on the conditions. This source of error will affect determinations of E.M.F. made by such methods as those used by Latimer Clark in the valuation of his standard cell (Proc. Roy. Soc. xx. p. 444), and will similarly affect any experiments on the determination of J by methods based on the same principles as those involved in Joule's 1867 observations, unless the mode of experimenting be modified in such a way as to eliminate this source of error, or at least to render it negligible. We propose to examine certain such modifications which have occurred to us as being likely to reduce this source of error to an inconsiderable amount.

XXX. *Determination of the Acceleration of Gravity for Tokio, Japan.* *By* W. E. AYRTON *and* JOHN PERRY.

As no experiments had, as far as we were aware, been made to determine the value of g in Japan, it appeared to us desirable that the value should be accurately measured, at any rate for our own college in the capital, Tokio. Consequently in 1877 an elaborate series of experiments was carried out by some of the students under our supervision. The method first employed consisted in experimentally finding two parallel axes in a pendulum on opposite sides of the centre of gravity, and in a plane with it, such that the times of oscillation about either axis would be the same. The distance, then, between these axes experimentally found would, as is well known, be the length of the equivalent simple pendulum, from which g could be calculated by the formula

$$t=\pi\sqrt{\frac{l}{g}}.$$

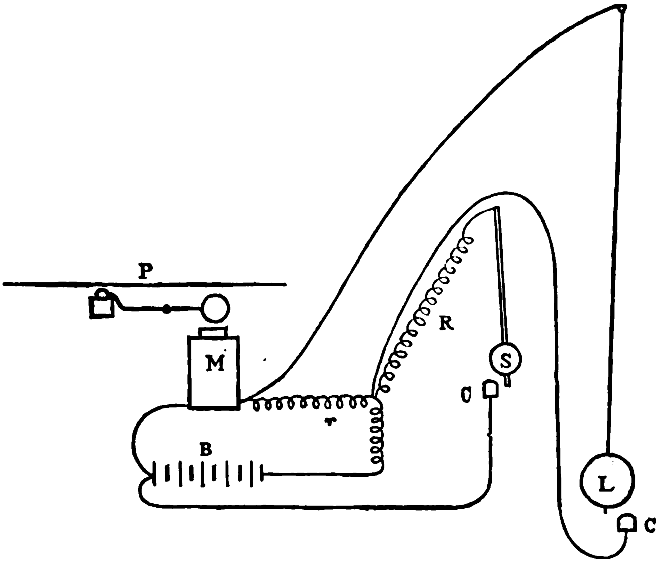
Two Kater's pendulums were employed—one made by Messrs. Elliott, and the other by Messrs. Negretti and Zambra. Borda's method of coincidence was employed; that is, one of the Kater's pendulums was suspended exactly in front of the pendulum of a clock, consisting of a wooden rod carrying a brass bob and beating approximately seconds. One observer watched the two pendulums, vibrating one in front of the other, through a telescope some ten feet away; and at the instant a pointer attached to one of the pendulums exactly coincided with a line drawn on the other (the coincidence taking place in the axis of the telescope as observed by the cross-wires), a signal was given, and the time of the clock noted by two independent observers. A very large number of successive coincidences was in this way observed, then the Kater's pendulum inverted, swung on the other knife-edge, and the same thing repeated. From these experiments the exact time of vibration of the Kater's pendulum about either knife-edge was ascertained, and one or both knife-edges moved to diminish the difference in the time of vibration, and the whole experiment repeated.

But although these observations, first with one of the Kater's pendulums and then with the other, were continued for some months, many thousands of vibrations being observed by the students, who worked at these experiments in their usual most praiseworthy way, the results were always unsatisfactory. For an approximate value of g for any part of the earth's surface can be calculated by a formula developed by Clairault, who from pendulum-experiments made at a variety of latitudes on the earth's surface has shown that, approximately, for any latitude λ and any height h centimetres above the level of the sea,

$$g=980{\cdot}6056-2{\cdot}5028\cos\lambda-0{\cdot}000003h.$$

As the latitude of the Imperial College of Engineering, Tokio, is about 35° 39′, Clairault's formula gives for g the value 979·7 centimetres per second per second. But the value obtained by the series of experiments made with the Kater's pendulums differed too much from this and from one another to allow of our trusting them.

We therefore decided on employing a totally different method and a much less laborious one:—A brass ball 2352·2 grammes in weight was suspended by a long steel wire 0·45 millimetre in thickness, and in the earlier experiments 978·7 centimetres in length. The wire was supported from a steel knife-edge resting on a brass plate. Both the brass ball and the bob of the seconds-pendulum of the standard clock were fitted with fine pieces of platinum wire, either of which dipped into a small cup of mercury when the pendulum to which it was attached was vertical. The mercury-cups &c. were then joined up with a battery and resistance-coils to a quick-running Morse instrument, as seen in the figure, in which M is the Morse instrument, B the battery, P the paper, S the seconds-pendulum, L the long pendulum, C, C the mercury-cups, R a resistance small compared with that of M plus that of the coil *r*, but large compared with that of M alone. The whole constituted what is known as a "break-circuit chrono-



graph;" that is, a continuous ink mark was made on the paper run out by clockwork, broken by a very small gap each time the wire attached to the bob of the seconds-pendulum passed through the mercury. These breaks, then, in the ink line indicated seconds; if, however, both pendulums were simultaneously in the vertical line, no break was made. Hence the absence of a break in the line at the end of any special second indicated coincidence of the two pendulums; and in this way the times of a large number of coincidences could be automatically registered.

During this set of experiments we could not measure the length of the long fine steel wire with as much accuracy as was desired, since, although we had two or three brass scales, the makers had omitted to record on them at what temperatures they were correct. However, assuming that one of them was accurate at 0° C., then a rather large number of experiments gave, as the value of g, 978·8 centimetres per second as a first rough approximation.

Subsequently we obtained from the Finance Department of Japan the loan of two very beautiful standard brass scales, by Deleuil of Paris, and guaranteed correct at 0° C. One was graduated in millimetres; the other consisted of a brass rod with two pieces at its ends at right angles to the rod, and the distance between the two planes of the inner surfaces of the pieces was exactly a metre at 0° C. We now had, then, the means of making a far more complete series of experiments than before; but as our trial pendulum was nearly ten times as long as the seconds-pendulum of our clock, the method of coincidences was an inconvenient one; and so we merely adopted the following:—The long pendulum alone controlled the "break-circuit chronograph;" so that the number of breaks in the line during any time indicated the number of vibrations of the long pendulum in that time. At the commencement of the experiment, after the pendulum had been set swinging and the paper was running out at a fairly uniform speed, a mark was made on it by tapping sharply the armature up with the finger when a chronometer, lying beside the Morse instrument, indicated a certain time; and after an hour or so, the paper being kept running all the time, a second mark was sharply made on the paper when the chronometer indicated a certain other noted time. So much paper had then run out in the interval of time shown by the chronometer; and the breaks in the line, counted carefully afterwards by two independent students, gave the whole number of vibrations of the pendulum in that time. The fraction of a vibration could also, of course, be ascertained by comparing with the length of the lines in the neighbourhood the length of the first line made after the first break had been produced, on tapping the armature, and repeating the same process at the end of the paper. The experiment is, of course, independent of the rate at which the paper runs out, provided, of course, it is never allowed to run so slowly that there is any difficulty in distinguishing the different breaks electrically made by the long vibrating pendulum. The mean temperature of the wire was carefully taken at each experiment. A sample of one or two of the many experiments recorded in the students' laboratory notebook follows.

Number of Experiment 26.—*25th of January,* 1878.

Number of vibrations obtained from counting on the paper 1015¾
Time taken—observed on the chronometer . 0 h. 52 m. 0 s.
Mean temperature at the beginning . . . . 10°·5 C.
" " " end . . . . 11°·25 C.
Time of vibration = 3·0744 seconds.

Number of Experiment 45.—*15th of February,* 1878.

Number of vibrations obtained from counting on the paper . . . . 1385·5
Time taken—observed on the chronometer . 1 h. 11 m. 0 s.
Mean temperature at the beginning . . . . 9°·25 C.
" " " end . . . . 12°·25 C.
Time of a vibration = 3·0745 seconds.

Number of Experiment 53.—*21st of February,* 1878.

Number of vibrations obtained from counting on the paper . . . . 1288·5
Time taken—observed on the chronometer . 1 h. 6 m. 0 s.
Mean temperature at the beginning . . . . 8°·5 C.
" " " end . . . . 12°·25 C.
Time of a vibration = 3·0741 seconds.

Frequent sun-observations were made to check the rate of the chronometer, which is comparatively easy in Japan, as during the winter there the sun is seen almost daily from sunrise to sunset.

The next point was to measure accurately the length of the wire. As it was impossible to do this satisfactorily with the wire hanging up, it was taken down without disconnecting either the knife-edge carrying it or the ball at the other end. The knife-edge was then fixed at one end of a horizontal rail, and the other end of the wire close to the ball hung over a wheel with very little friction. By this arrangement the wire in a horizontal position was, of course, stretched as much as it was in the vertical position, as far as the effect of the weight of the ball was concerned. A correction had, however, to be made for the weight of the wire itself, which of course caused the tension to be a little less at the bottom than at the top when the pendulum was hanging up vertically. A few centimetres of similar fine steel wire being weighed, a simple integration gave the small additional weight necessary to be added. This being done, the final result obtained was that the length of the pendulum equalled 939·09 centimetres at 0° C.; and the consequent value of g in air for Tokio, Japan, calculated from the result of about eighty thousand vibrations of the long pendulum, would be 980·06 centimetres per second per second, if the pendulum could be regarded as a simple mathematical pendulum.

Correcting Factors.

1. The two most obvious corrections to apply to this result are the corrections for infinitely small arcs and for the air-friction—neither of which were found of any practical consequence, on account of the very small angle through which the pendulum usually swung, and that the decrement of the amplitude of the vibrations was imperceptible even after many swings. Although, however, such a pendulum as we were using approaches very nearly a perfect simple pendulum, there are certain causes of possible error arising from its flexibility and slight elasticity which would not affect a rigid compound pendulum. To estimate the practical effect of these possible errors, it is necessary to solve generally the complete problem

of a heavy ball supported by an elastic wire, one end of which is soldered to the ball and the other end to a steel knife-edge. When a suspended ball is swinging in the arc of a circle, we know that near the end of a swing the attachments of the ball have to resist a tendency for the ball to turn. For since the ball has been turned in passing from its lowest to its highest position, it would continue to turn were it not stopped by the wire itself. At the end of every swing, then, there must be a slight kick; so that in fact the ball will make minor swings about its point of attachment all the time of the motion. To make this kick less perceptible, we must make the fastening of the wire to the ball capable of resisting the tendency of the ball to continue its turning motion. If we do this by soldering the wire, a smaller kick will result, and will be due to the bending-moment of the wire resisting the turning action. If there were no difficulty of construction, it might be better to get rid of this kick difficulty by making the bob capable of rotating in the plane of swinging about an axis through its centre of gravity.

The investigation of the general problem of the swinging of a heavy ball soldered to an elastic wire, the upper end of which is attached to a knife-edge, may take somewhat the following form:—Let A be a ball, of mass m and radius a, suspended from a free hinge at B by a chain of $n-1$ links, each of length a and hinged to one another, the last hinge being on the surface of the ball. Suppose at any hinge where two adjacent links make an angle θ with one another, equal and opposite couples act in them of moment $c\theta$ tending to bring them into the same straight line. As $n$ is made greater and greater, we approximate more and more nearly to our actual case of an elastic wire. Let $n$ be very great, and $\phi_1, \phi_2, \ldots \phi_{n-1}$ the inclinations of the 1st, 2nd, ... $(n-1)$th link to the vertical, the $n$th link being the radius of the ball up to the hinge, and its inclination ϕ_n. If now we know the mass of the links per unit of length, it is easy to state the values of $\psi_1, \psi_2, \ldots \psi_s, \ldots \psi_n$, the couples acting on each respective link: thus

$$\psi_n = mga \sin \phi_n + c(\phi_n - \phi_{n-1}),$$

care being taken to remember that the form of ψ_s is different from that of ψ_1 or of ψ_n.

If the inclination ϕ is everywhere very small, we find, if v_s is the velocity of the end of link s, that

$$v_s^2 = a^2\{(\dot{\phi}_1)^2 + 2\dot{\phi}_1\dot{\phi}_s \cos(\phi_s - \phi_1) + (2s-3)(\dot{\phi}_s)^2\},$$

where $\dot{\phi}_s$ means $\frac{d\phi_s}{dt}$.

So that the kinetic energy T of the whole system may at once be written out in terms of the coordinates $\phi_1, \phi_2, \ldots$ &c., $\dot{\phi}_1, \dot{\phi}_2, \ldots$ &c.

We can therefore find the partial differential coefficients

$$\frac{d\mathrm{T}}{d\dot{\phi}_1}, \ \frac{d\mathrm{T}}{d\dot{\phi}_2}, \text{ \&c., and } \frac{d\mathrm{T}}{d\phi_1}, \ \frac{d\mathrm{T}}{d\phi_2}, \ldots \text{ \&c.},$$

so as to use Lagrange's equation

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{d\dot{\phi}}\right) - \frac{d\mathrm{T}}{d\phi} = -\psi.$$

In this way we have obtained the n differential equations connecting $\phi_1, \phi_2, \ldots \phi_n$, and their first and second differential coefficients with respect to t. As, however, these equations can only be regarded as true when n is infinite, and as the labour of solution is very great when n is great, it seems useless proceeding further with the solution.

If we regard the motion of the ball as a harmonic motion of period P, determined by assuming the connexions as rigid, combined with motions of much shorter periods $\mathrm{P}_1, \mathrm{P}_2, \mathrm{P}_3, \ldots$ &c., there will be some little difficulty in finding the motion of shortest period P_1, namely the *kick* above mentioned; but we know that when the wire is, as in our experiments, very thin, the kick cannot be much less than the time of a complete vibration of the ball when freely suspended by a point on its surface, or

$$2\pi\sqrt{\frac{12a}{5g}},$$

where a is the radius of the ball. But this periodic time is 0·528 seconds, or about one twelfth of that of the pendulum moving as a whole, which is about 6 seconds.

Since the tendency of the ball to add this quick vibration to its motion is due to its rotational energy, it may be diminished by lessening the moment of inertia of the ball (that is, by making the ball small), or by diminishing the angular velocity of the pendulum (that is, by making the pendulum as long and its swing as small as possible). We may regard, then, the motion of the ball as compounded of a pure harmonic motion with an amplitude of about 30 centimetres and a periodic time of 6 seconds, with another motion having a very small amplitude and with a period of about half a second. But we have proved in the paper on our seismograph*, that in such a case the compound motion would differ very slightly from that of a pure harmonic motion, even if there were no internal friction in the substance of the wire (supposing the pendulum started without shock); but as internal friction, of course, exists in the wire, this error becomes exceedingly small.

2. Next, with regard to the stretching of the wire arising from variations in the centrifugal force of the ball while swinging. Since the time of a complete vibration of our pendulum was nearly 6 seconds and the arc about 30 centimetres, the velocity at the middle of its path was

$$\frac{30 \times \pi}{6}, \text{ or } 15{\cdot}7 \text{ centimetres per second};$$

hence the pull on the wire, which at the end of the swing was equal to the weight of the wire, or 2352·2 grammes, was increased by

$$\frac{2352{\cdot}2 \times (15{\cdot}7)^2}{939 \times 979{\cdot}7} \text{ grammes}$$

at the middle of the swing. But this is less than a gramme, so that no practical extension of the wire arose from centrifugal force.

3. Shortening of the length of the wire, due to its curvature, arising from the resistance of the air making it concave in the direction of motion. It is easy to see that the shortening of the pendulum due to this cause is excessively small, and is of the same order as the lengthening arising from the centrifugal force; so that these two very small errors may be regarded as balancing one another.

Also, since it may be calculated that the period of transverse vibration of the wire is less than one fortieth of the periodic time of the pendulum, the resistance of the air cannot tend to cause amplification of the lateral vibrations in the wire itself.

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* "On a Neglected Principle that may be employed in Earthquake Measurements," Trans. Asiatic Soc. of Japan, vol. v. part 1, p. 181; reprinted in Phil. Mag. July 1879.

We may therefore assume that our pendulum vibrated like a rigid body, consisting of a ball of brass, a straight steel wire, and a triangular steel prism of which the edge was the fixed axis.

Calculation of g.

The complete formula is, of course,

$$t=\pi\sqrt{\frac{\Sigma(mr^2)}{l\,.\,mg}},$$

l being the distance from the axis of rotation to the centre of gravity of the pendulum.

The steel knife-edge had a length of about 4 centimetres, a breadth of about 1 centimetre, and a depth of ½ a centimetre; hence its weight was about 7·8 grammes, its moment of inertia about the axis of rotation 0·98 (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 0·33 centimetre. The weight of the wire was 11·6 grammes, and its length 934·99 centimetres at 0° C. Its moment of inertia was therefore $3{\cdot}3803\times10^6$ (gramme, centimetre), and the distance of its centre of gravity from the axis of rotation 467·49 centimetres. The weight of the brass ball was 2352·2 grammes, its moment of inertia about the axis of rotation $2{\cdot}0744\times10^9$, and the distance of its centre of gravity 939·09 centimetres at 0° C. Of the whole system, then, the weight was 2371·6 grammes, the moment of inertia about the axis of rotation $2{\cdot}0778\times10^9$ (gramme, centimetre), and the distance of its centre of gravity $2{\cdot}2144\times10^6$. Consequently

$$g=\left(\frac{\pi}{t}\right)^2\frac{2{\cdot}0778\times10^9}{2{\cdot}2144\times10^6},$$

and

$$t=3{\cdot}0748 \text{ seconds};$$

or

$$g=979{\cdot}58 \text{ centimetres per second per second in air,}$$

or

$g=979{\cdot}74$ *centimetres per second per second* in vacuo *for the Imperial College of Engineering, Japan*—

a result agreeing extremely closely with the number 979·7 obtained above from Clairault's formula.

In beginning this series of observations we expected to find g to be greater than what Clairault's formula gives it. Our reason was this:—Clairault's formula assumes a circular equator; Capt. Clarke has found that the equator is elliptical, one extremity of its major axis being in 15° 34′ E. longitude; and therefore Tokio is in longitude nearer a minor axis than a major one. We find, however, a reason why g satisfies so well Clairault's formula, in spite of this excentricity of the equator. The greatest depression of the earth's surface is only a few hundred miles to the east of Japan; and probably the diminution in g produced by this cause just counterbalances the increase of g produced by ellipticity of the equator. As for local perturbations, it is to be remarked that Tokio is situated on a very large plane, there being no hills of any magnitude within eighty miles. We think that the geodesy of Japan is of special interest on account of the great Pacific depression, and on account of the very gradual slope of the earth's surface from Japan to China, which causes Japan to be a sort of ridge.

Probably the best method of determining the value of g would be to use a *rigid* homogeneous pendulum, of such a shape that its moment of inertia could be easily calculated with accuracy from its linear dimensions measured at any temperature—for example, a sphere at the end of a cylindrical rod with a knife-edge, all cast in one piece and turned true in the lathe, or a cylindrical bob cast at the end of the cylindrical rod. The only possible objection to this method would be the possible want of homogeneity in the metal. This might be allowed for in the following way:—Instead of casting the cylindrical bob in one piece, make it consist of a number of concentric tightly fitting cylindrical shells accurately turned. Experiments would then be made first with the cylinders all in one position, then with some of them twisted slightly round, and so on until in the mean result the errors of eccentricity of mass would probably be eliminated.

Another, and perhaps the best of all methods, would be for rigid compound pendulums to be accurately timed experimentally at Greenwich at a number of different temperatures, and sold with a scale of temperature-corrections for the time of vibration attached.

One set of experiments, then, with one of these pendulums anywhere would at once give the value of g; and such a pendulum would undoubtedly be the most suitable for surveys and expeditions in foreign countries. The fact is, the mathematical beauty of the principle involved in the Kater's pendulum has, in our opinion, caused far too much importance to be attached to it as a practical instrument for determining experimentally the value of the acceleration of gravity.

We have to thank several of our late students, and especially Messrs. Honda, Kikkawa, A. Kasai, J. Nakahara, and H. Nobechi for assistance rendered us during this investigation. And it may here be mentioned that this investigation, like the many others we have been enabled to carry out during the last few years, has resulted from the plan we have followed of teaching the laboratory students not, as is customary in Colleges, to repeat well-known experiments, but to endeavour in their investigations to advance, in some small degree at any rate, the bounds of existing knowledge. And this system of enlisting the assistance of even quite young students in original research we have found to create an enthusiasm in experimental work otherwise unproducible.

XXXI. *On Induction in Telephonic Circuits.* *By* W. Grant[*].

The apparatus used in the following experiments was such as was ready to hand in the laboratory. The particulars given are intended merely to show the conditions under which the results described were obtained; there is no reason to suppose that moderate, or perhaps even considerable alterations in the dimensions of the apparatus would have caused the results to be essentially different.

One of the appliances used is a coil which consists of two similar No. 20 copper wires of equal length wound side by side throughout, forming a coil of 2·3 centimetres internal, and 12·7 centimetres external diameter, and about 12·5 centimetres in length. The resistance of the two wires connected end to end is 28 ohms. This is afterwards referred to as the double helix.

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[*] Read January 24th, 1880.

Two other coils, afterwards spoken of as "flat" coils, consist of approximately equal lengths of No. 19 copper wire, wound in rectangular grooves in two flat wooden reels. The inside and outside diameters of these coils are about 10 centimetres and 15 centimetres respectively. The resistance of each is roughly 1 ohm.

Two rough coils of copper wire, in the state in which they came from the manufacturer, were also employed in the first experiment described; but as they are not afterwards referred to, their dimensions need not be given.

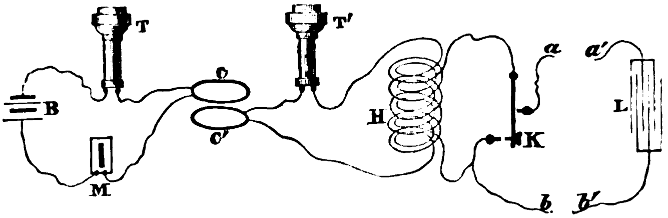
If four separate circuits are so arranged that the primary one includes a battery, a microphone which is actuated by a watch, a telephone, and one of the wires of the double helix—the secondary includes the other wire of the double helix, a telephone, and a rough coil—the tertiary includes another rough coil in proximity to that in the secondary circuit, a telephone, and a flat coil—the quaternary includes another flat coil in proximity to that in the tertiary circuit, and also a telephone,—a current of a different order circulates in each of the separate circuits, and the following effects are observed.

The loudness of the sounds in the telephones in the primary and secondary circuits is as nearly as possible the same, in fact no difference can be detected between them; but in the tertiary and quaternary circuits the sounds are not so loud, partly no doubt because the mutual induction between the secondary and tertiary circuits, and between the tertiary and quaternary, was less than that between the primary and secondary, seeing that the coils by whose mutual induction the effects were transferred from the secondary to the tertiary circuit, and from the tertiary to the quaternary, were wound separately, and consisted of a smaller length of wire than the two which were wound side by side to form the double helix used to transfer the induction from the primary to the secondary circuit.

The pitch of the sound, however, is found to be the same in all the four circuits. Now, if the rate of the pulsations of induced currents is greater as the order of the current is higher, and if the telephone is capable of vibrating in unison with these pulsations, it follows that the pitch of the sound should be higher in some of the telephones than it is in others. But as the pitch of the sound is the same in all, either the telephones do not vibrate in unison with the rapid pulsations of the currents of high order, or the sameness of pitch must be due to some other cause.

The arrangement of apparatus in the next experiment is shown in fig. 1.

Fig. 1.



There are three separate circuits. The primary includes a battery B, a telephone T, a flat coil C, and a microphone M; the secondary includes a flat coil C′, a telephone T′, and one of the wires of the double helix H; the tertiary includes the other wire of the double helix and a contact-key K. L is a condenser.

The coils C and C′ are placed with their faces in contact and their axes coincident; and the microphone is actuated by a watch.

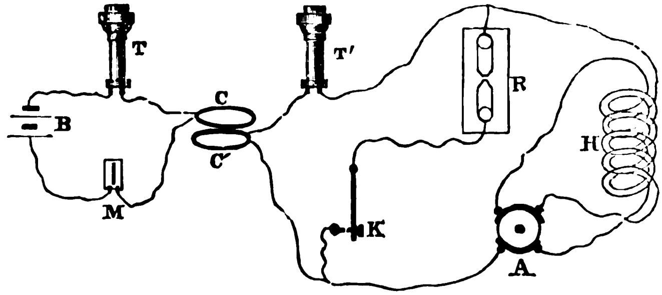
When a battery-current circulates in the primary circuit under these circumstances, currents are induced in the secondary and tertiary circuits, and it is observed, when the ear is applied to the telephone in the secondary circuit, that the loudness of the sound is increased when the key is depressed and the tertiary circuit is closed, and that it again becomes weak when the key is raised and the tertiary circuit is opened. The increase in the loudness is most readily observed when the sound is rather faint when the tertiary circuit is open; for if the sound is already loud the increase is not so marked.

The increased loudness seems to be due to an increase in the strength of the secondary current, caused by the reaction upon it of the currents induced in the tertiary circuit when the key is depressed. When the tertiary circuit is open, the self-induction of the secondary circuit opposes the variations of the secondary current; but when the tertiary circuit is closed, the mutual induction between it and the secondary circuit neutralizes to a great extent the effect of the self-induction of the secondary current, the strength of which is increased or augmented by the inverse currents in the secondary circuit, due to the reaction of the tertiary current upon it.

If the condenser L, fig. 1, is connected to the tertiary circuit by joining the wires at *a a′*, and *b b′*, it will be brought into action when the key is raised, and short-circuited when the key is depressed. If its capacity is three quarters or half a microfarad, the sound in the telephone T′ retains its normal pitch when the key is depressed; but when the key is raised and the condenser is brought into action the pitch of the sound rises; and the rise is greater with half than with three quarters of a microfarad. With a capacity of from about one to five microfarads, the sound retains its normal pitch.

The arrangement for the following experiment will be intelligible from fig. 2.

Fig. 2.



The primary circuit is the same as that in fig. 1, where B, T, C, and M, represent the battery, telephone, coil, and microphone respectively. The secondary circuit includes the coil C′, the telephone T′, the double helix H, and a commutator A, by means of which the direction of the current can be reversed in one of the wires of the helix. K is a key which, when it is depressed, short-circuits the double helix; R is a box of resistance-coils.

If the commutator is so placed as to cause the current to pass through both wires of the double helix in the same direction, the self-induction is strong, the current is weakened by it, and the sound in the telephone T′ is weak in consequence. If the current passes in opposite directions through the two wires of the helix (or returns on itself as in a resistance-coil), the self-induction is eliminated, and the current, having only to overcome the resistance of the circuit, is stronger than before, and the sound in the telephone T′ is considerably louder in consequence.

If the self-induction has been eliminated and the sound is already loud, no further increase in the loudness is observed when the key is depressed and the double helix is short-circuited. If the self-induction has not been eliminated, and the sound is weak, it becomes loud when the key is depressed and the double helix is short-circuited, and it requires 3000 ohms to be introduced at R in order that the loudness may be the same whether the current passes through the resistance-coils and key or through the double helix. With a larger pair of coils substituted for the flat coils C and C′, it requires 5000 ohms to be introduced at R in order to produce a similar result. As the resistance of the double helix is only 28 ohms, these results show, on the one hand, the deleterious effects which are produced by the self-induction of even a small coil, and, on the other, the importance of eliminating induction in conducting-circuits.

In the following experiments speech was transmitted by Bell's telephone, and two observers were employed, one at either end of the line.

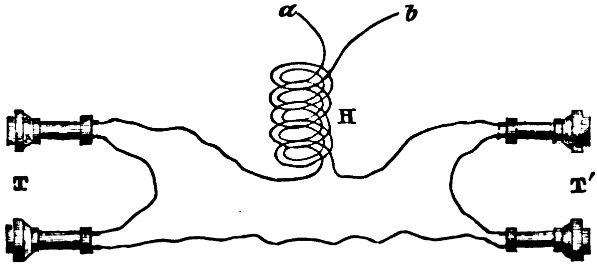
The coils, keys, &c. were placed at one end of the line, and were under the control of one observer, whose chief duty was to read a paragraph of a newspaper in a monotone the loudness of which was kept as nearly as possible constant, so that the inflections of the voice might not be mistaken by the observer at the other end of the line for an increase or diminution in the loudness of the tone resulting from the manipulation of the apparatus while the reading was progressing.

When a single circuit was employed which included one wire of the double helix, the loudness of the sound increased when the other wire of the helix was joined up as a separate circuit—a result similar to that obtained (as already mentioned) by joining up the tertiary circuit fig. 1.

When two separate circuits were employed, each of which included one of the wires of the double helix, the helix acted as a "relay," and messages were transferred or translated from one circuit to the other with perfect facility from either end of the line, and conversation was carried on with the same distinctness as when a single circuit only was employed.

A single circuit was arranged as shown in fig. 3.

Fig. 3.



T and T′ are the telephones, H is the double helix. The helix was joined up as shown in the figure, one end of each of its wires, *a* and *b*, being left unconnected and insulated. Thus arranged, there was a break in the continuity of the circuit, but nevertheless speech was transmitted with perfect distinctness, and conversation carried on with the same facility as in the previous experiments, the helix acting not as a conductor, but as a condenser, its two wires replacing the two sets of plates; and it made no difference which of the ends of the wires of the helix were connected to the line-wires, or whether the currents entered the wires of the helix in the same or in opposite directions.

The helix in the last case having been replaced by a condenser, the other arrangements remaining unaltered, speech was again transmitted with as much facility and clearness as if the circuit had been complete—the capacity of the condenser, which was one microfarad, being amply sufficient to allow conversation to be carried on.

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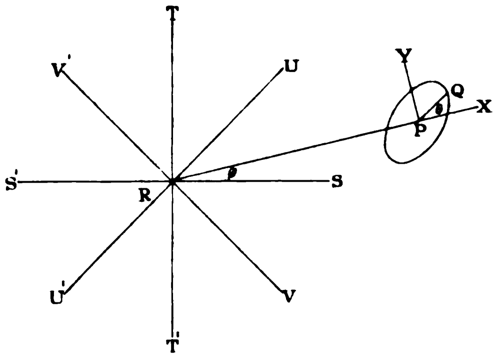

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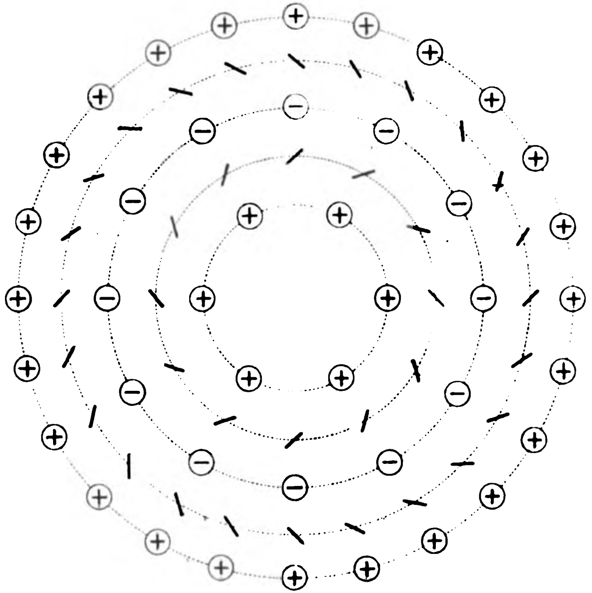

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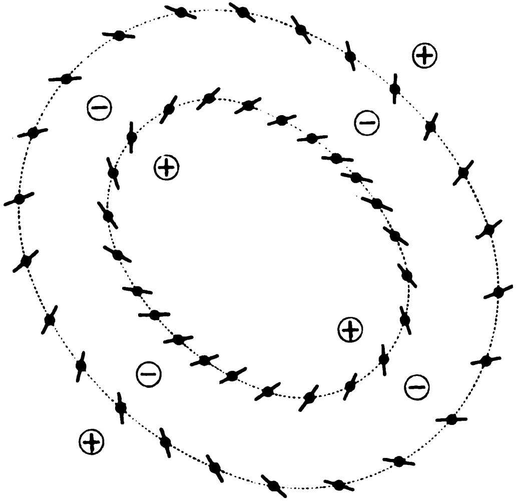

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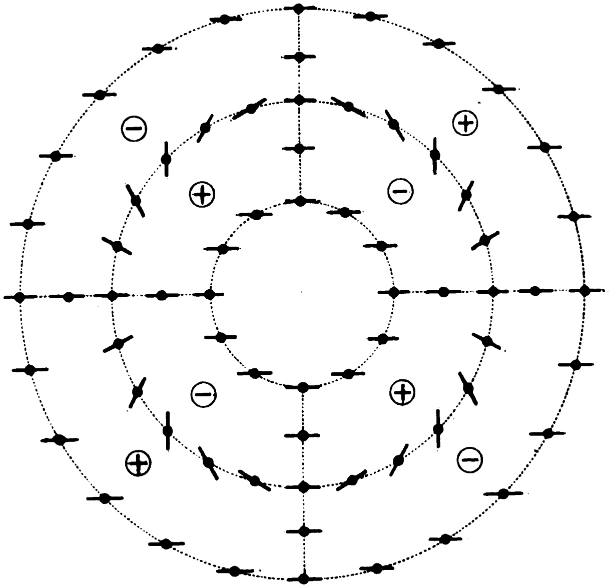

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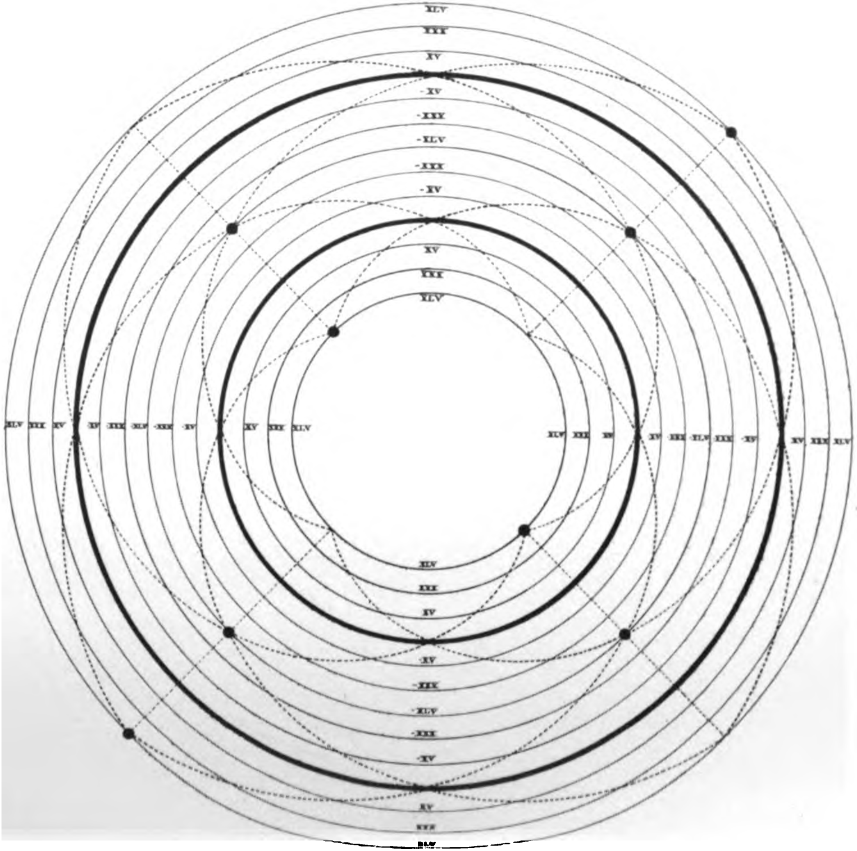

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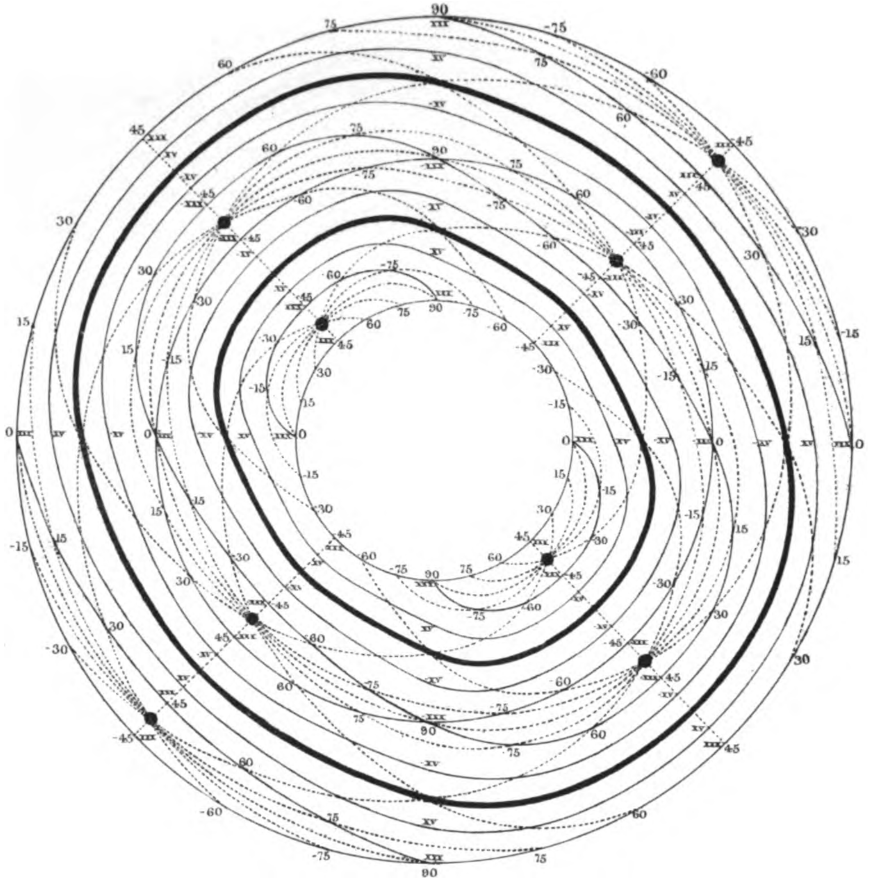

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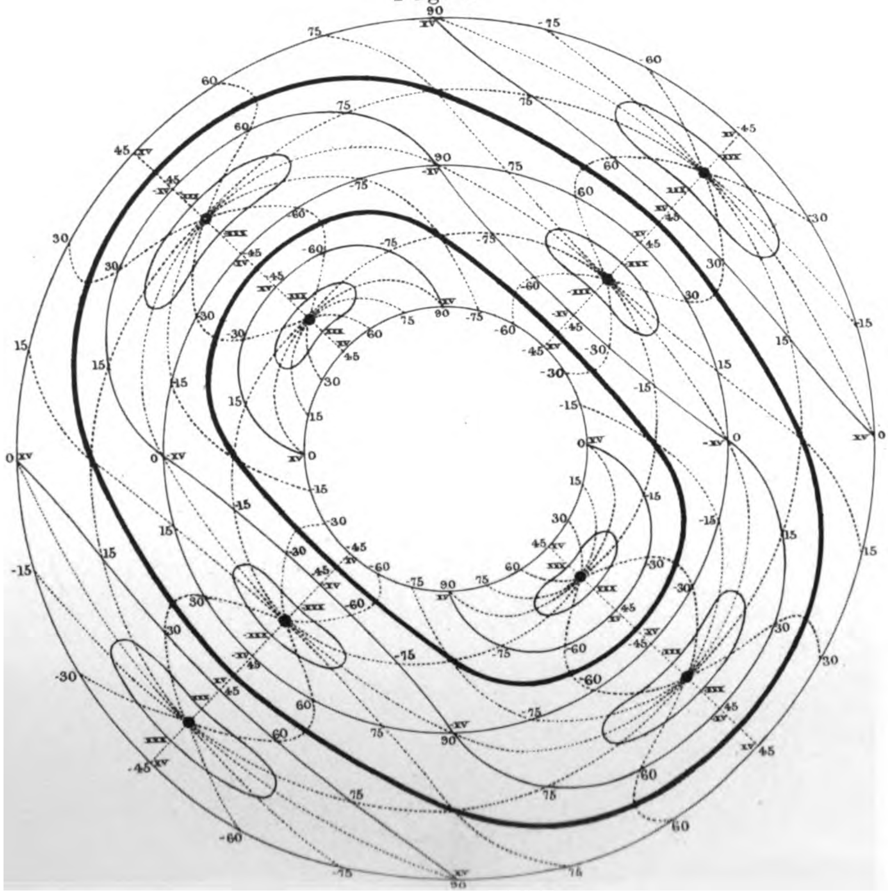

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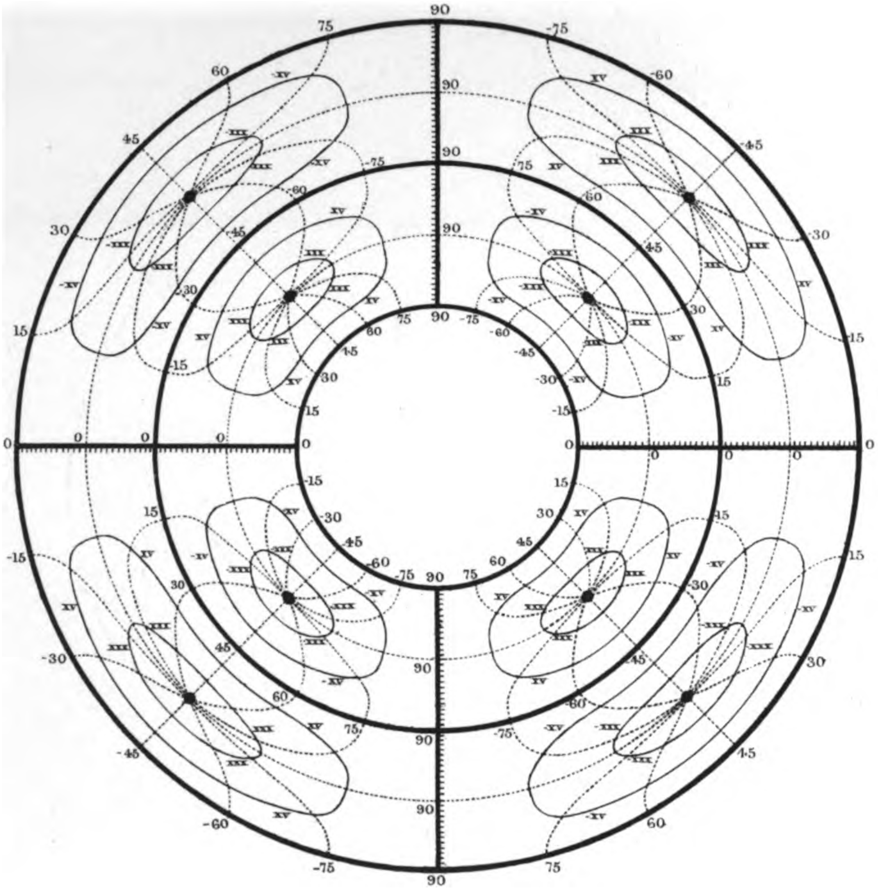

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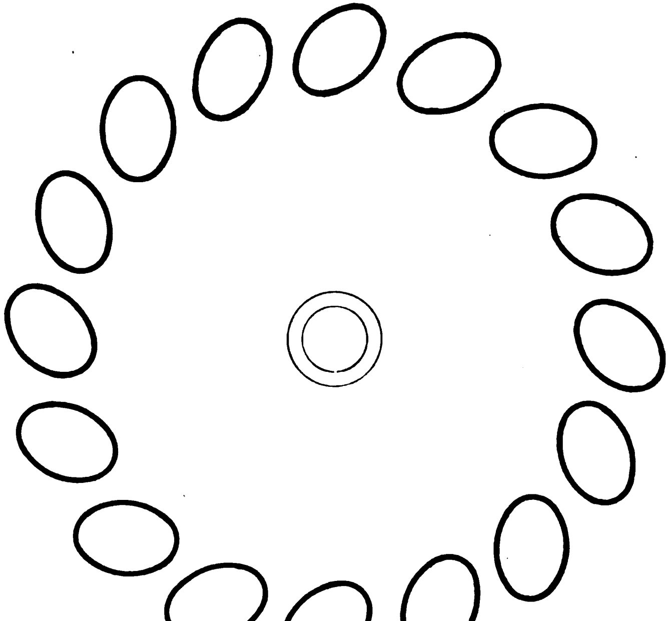

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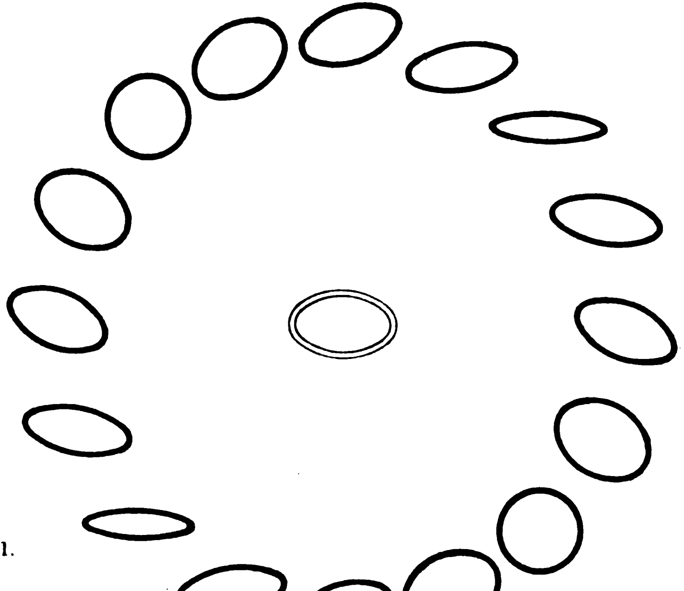

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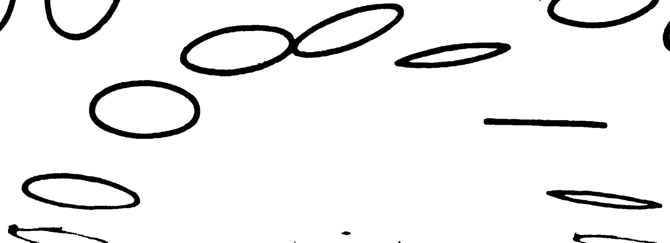

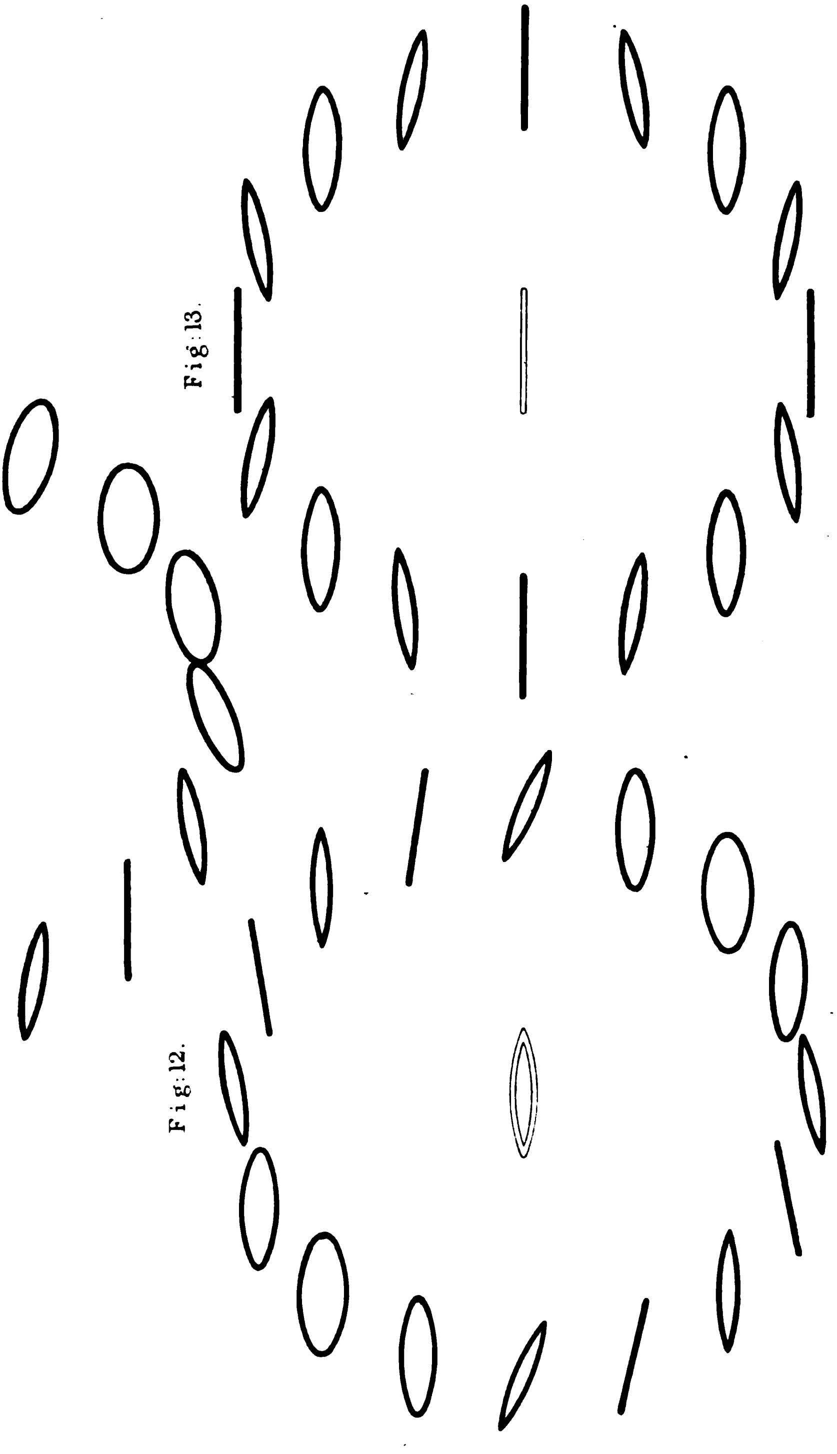

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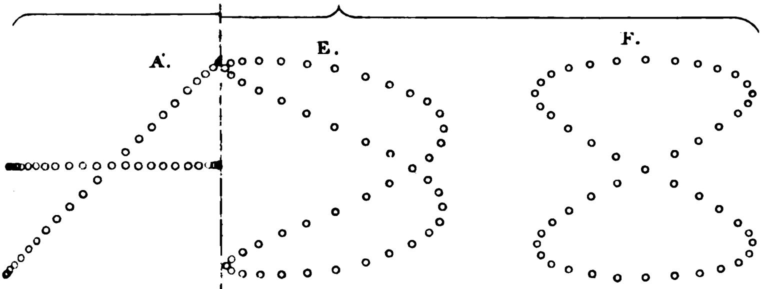

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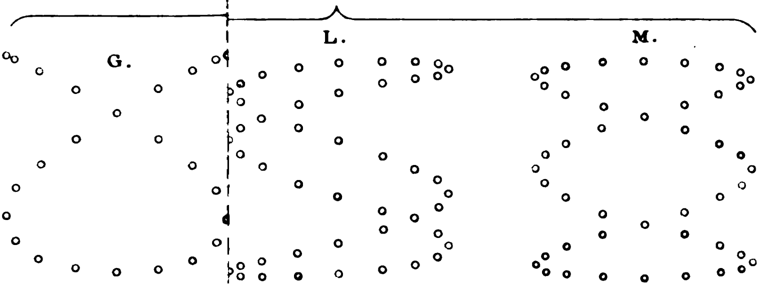

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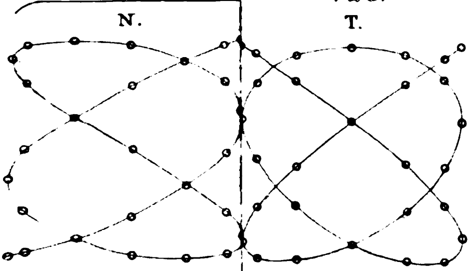

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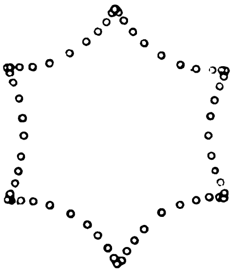

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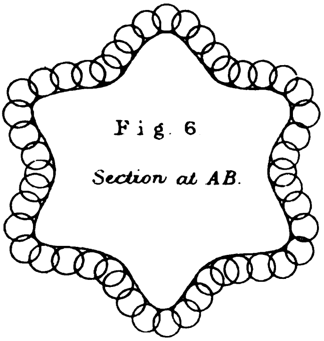

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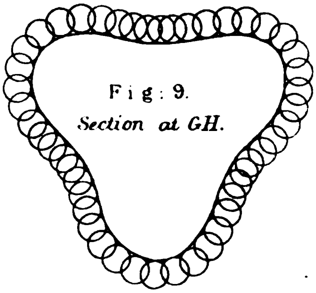

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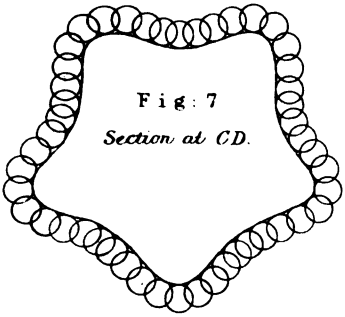

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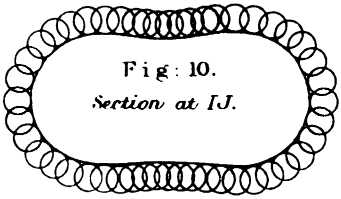

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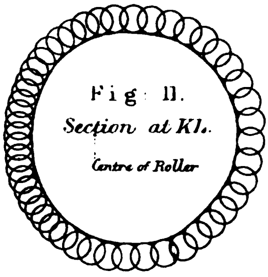

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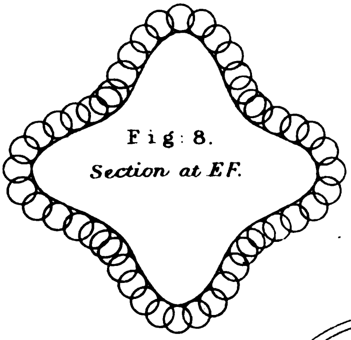

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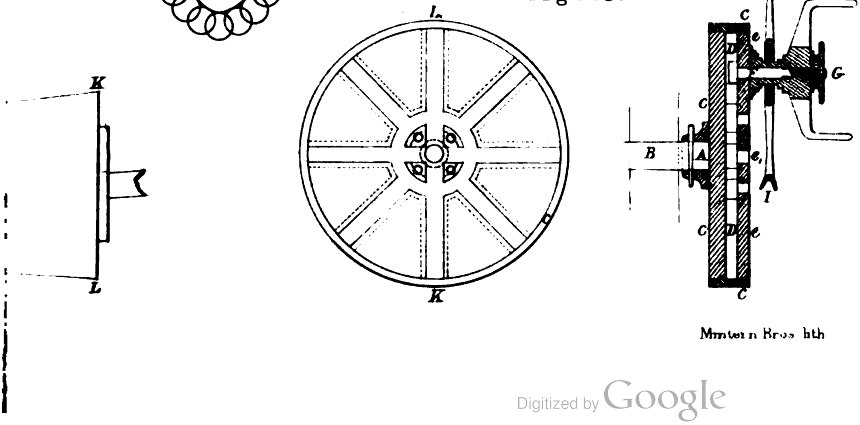

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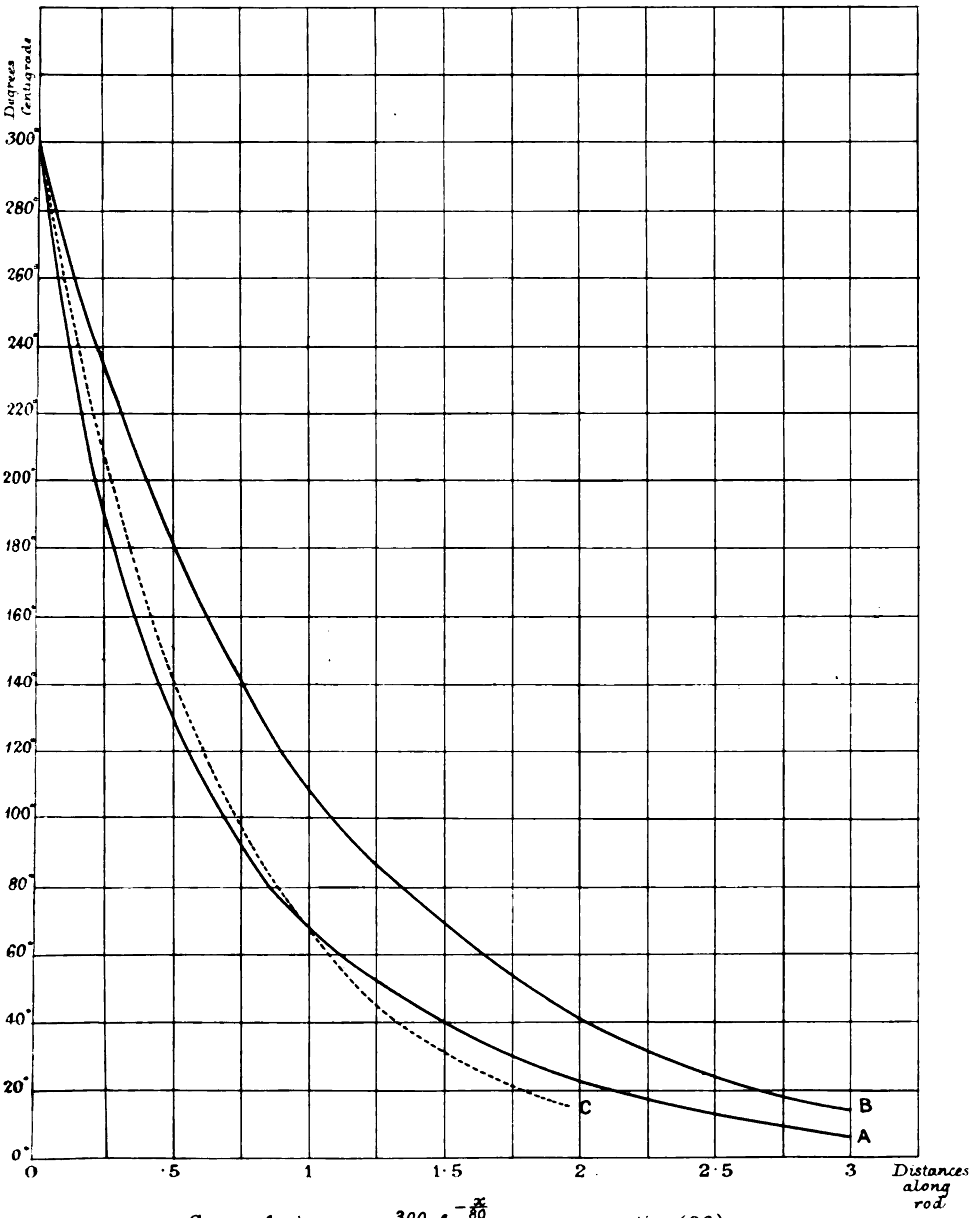

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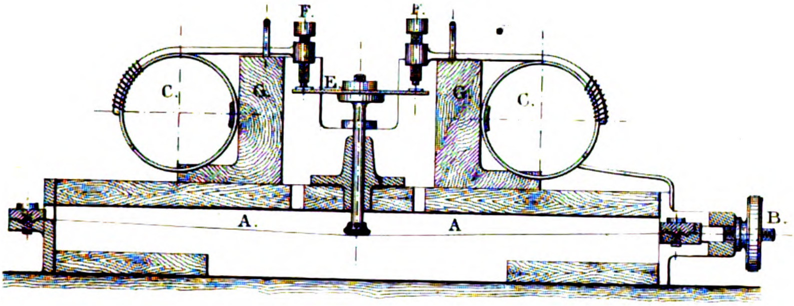

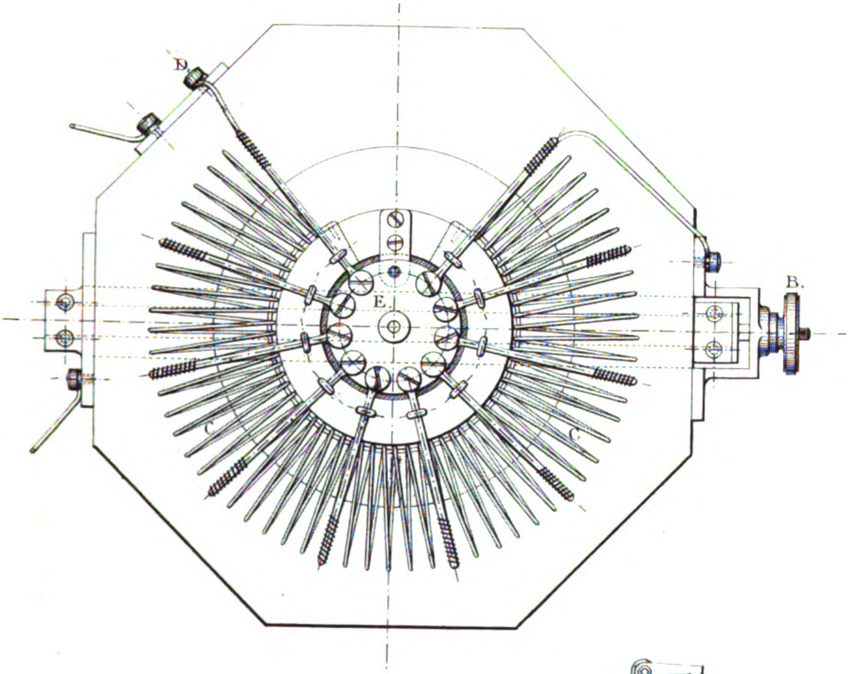

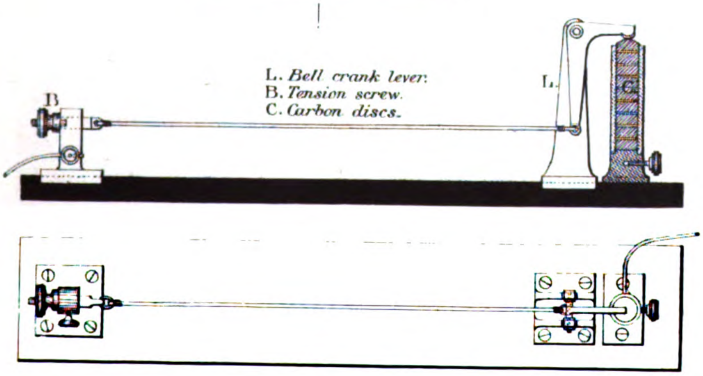

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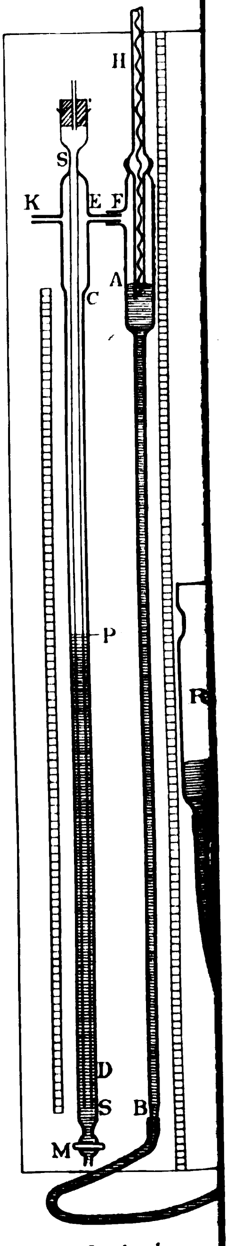

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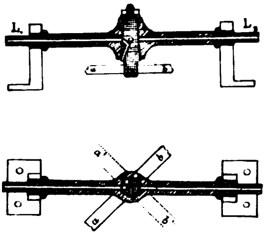

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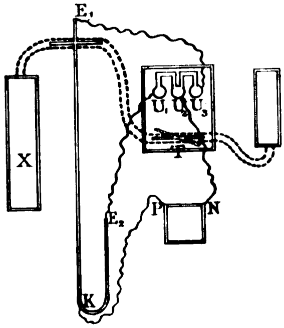

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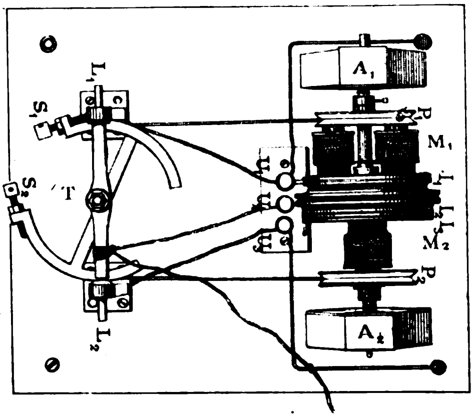

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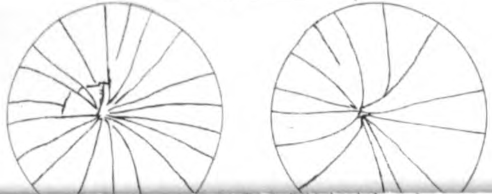

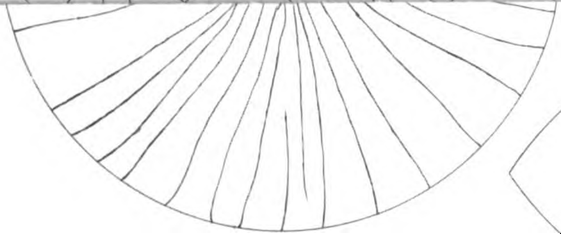

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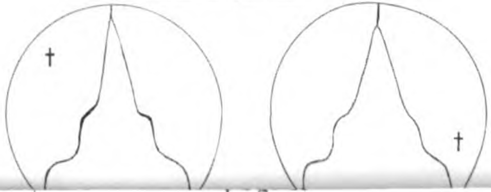

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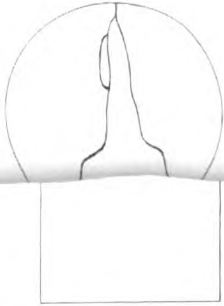

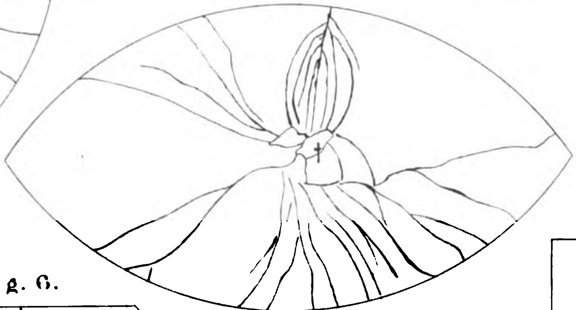

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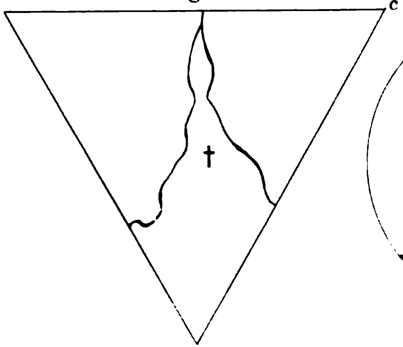

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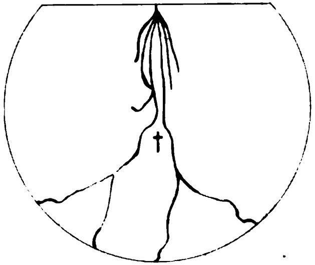

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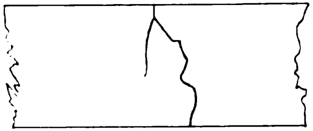

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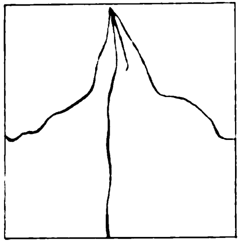

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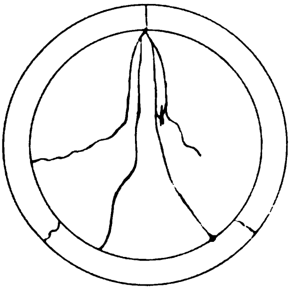

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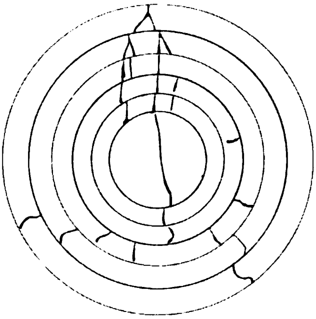

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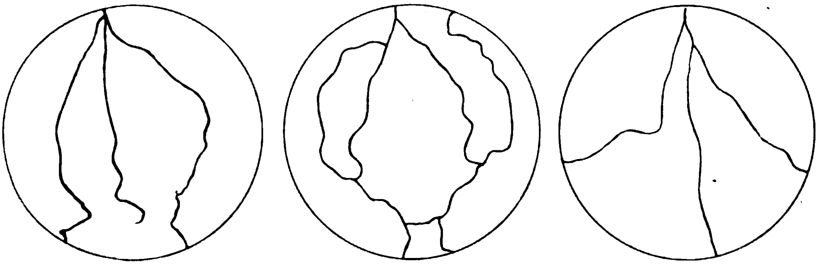

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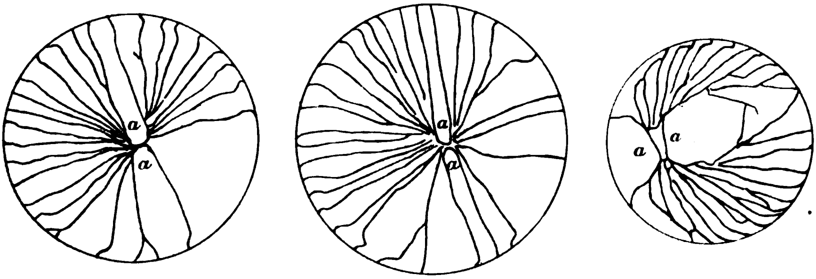

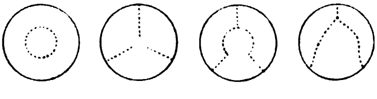

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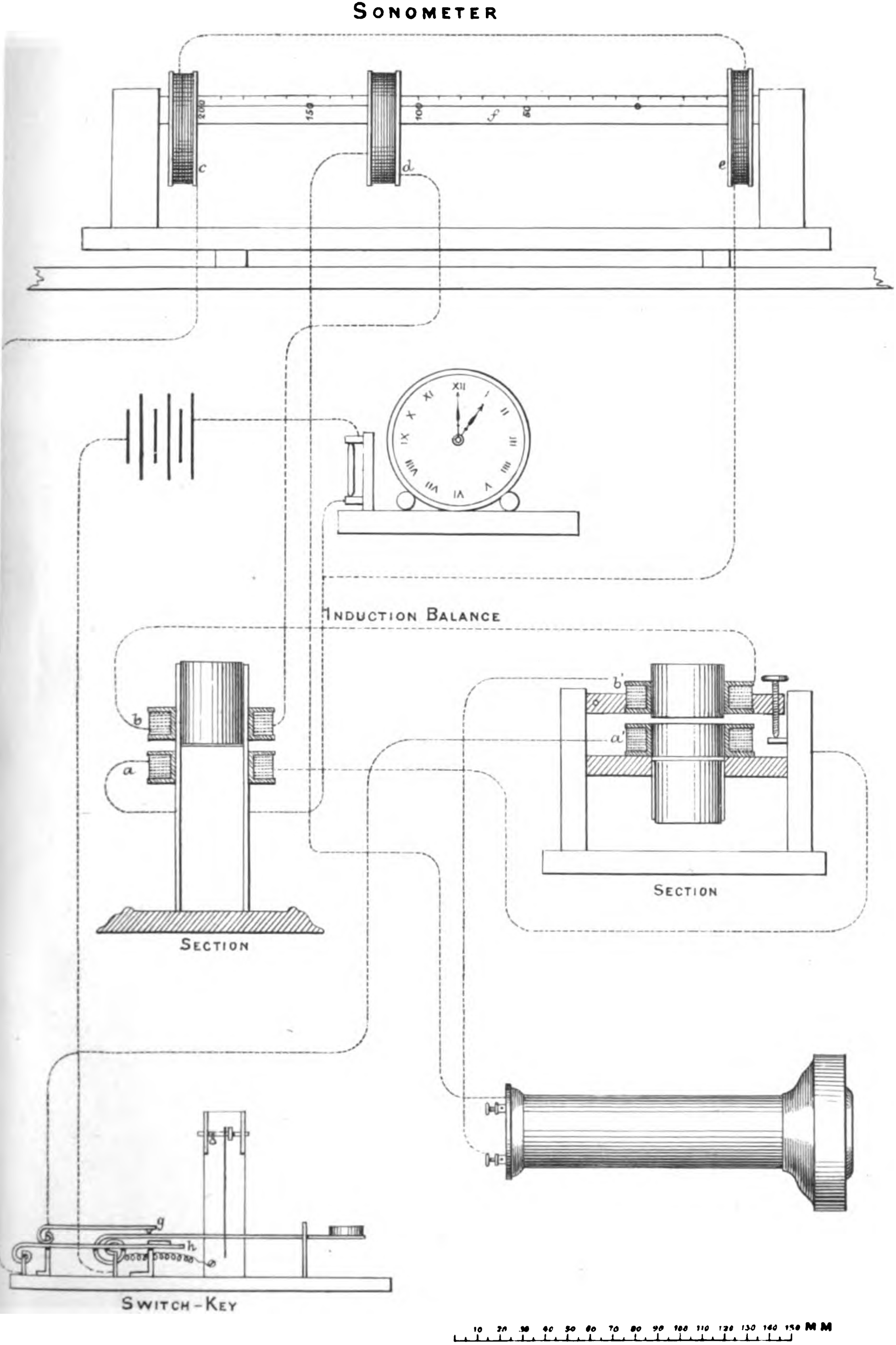

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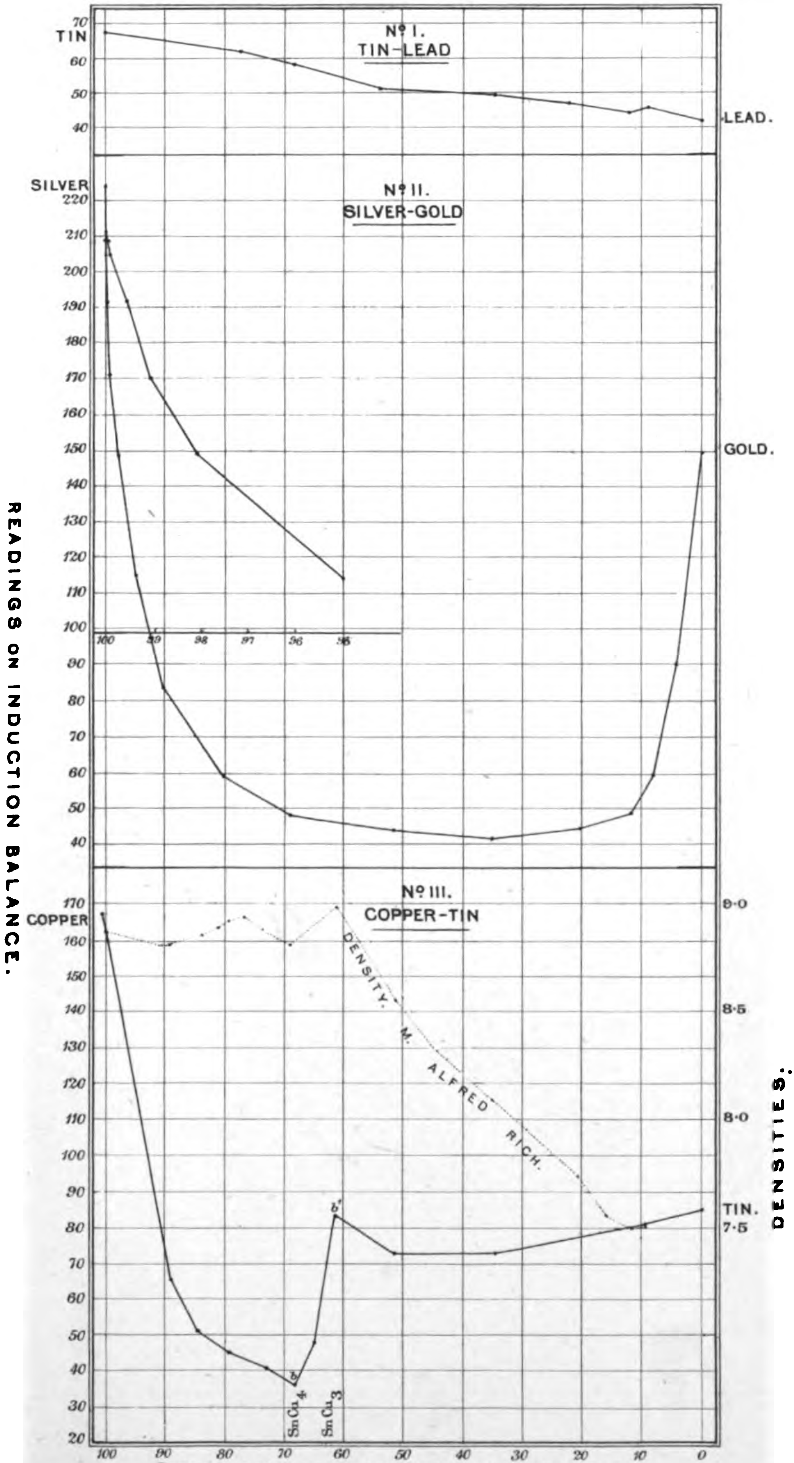

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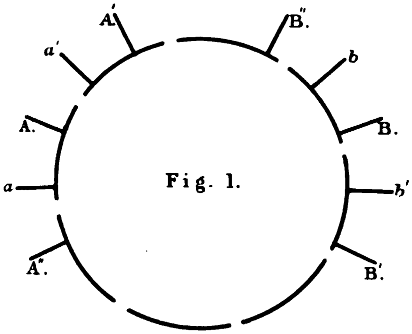


Fig. 1.

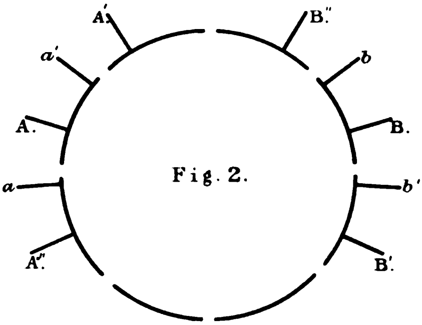


Fig. 2.

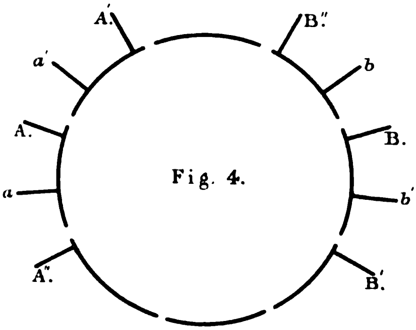


Fig. 4.

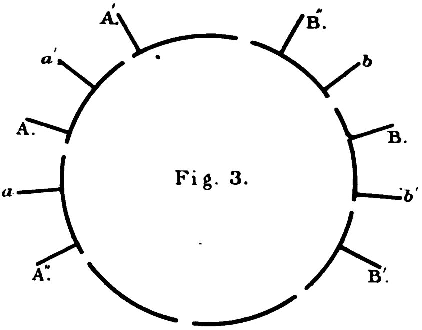


Fig. 3.

Mintern Bros lith.

Fig. 5.

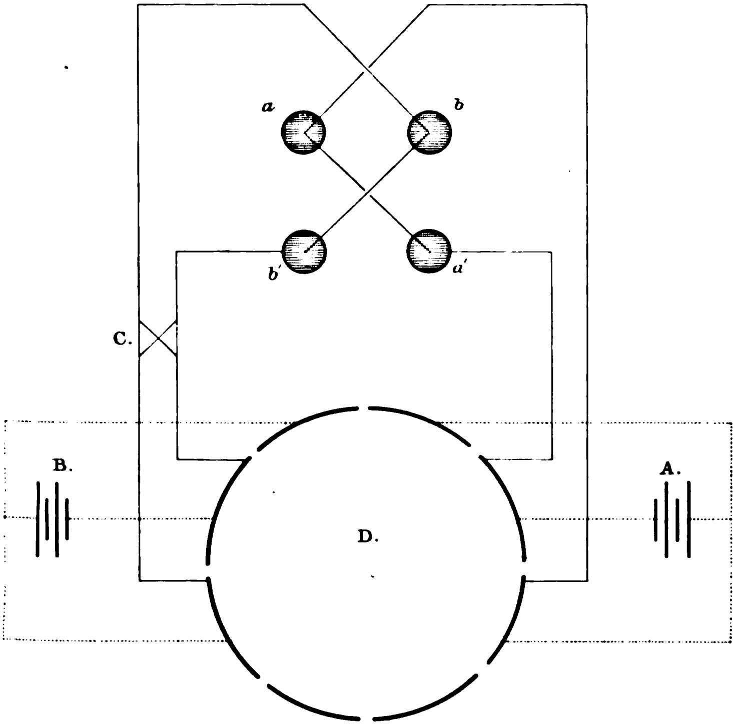
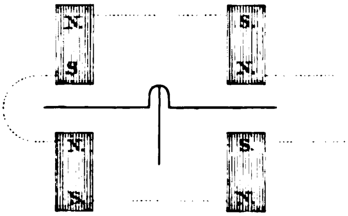


Fig. 6.



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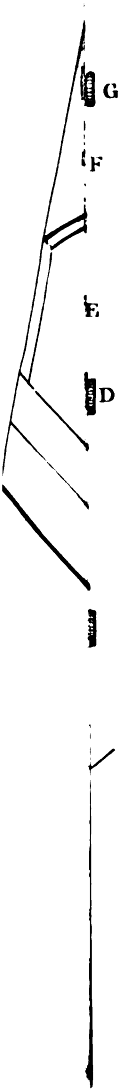
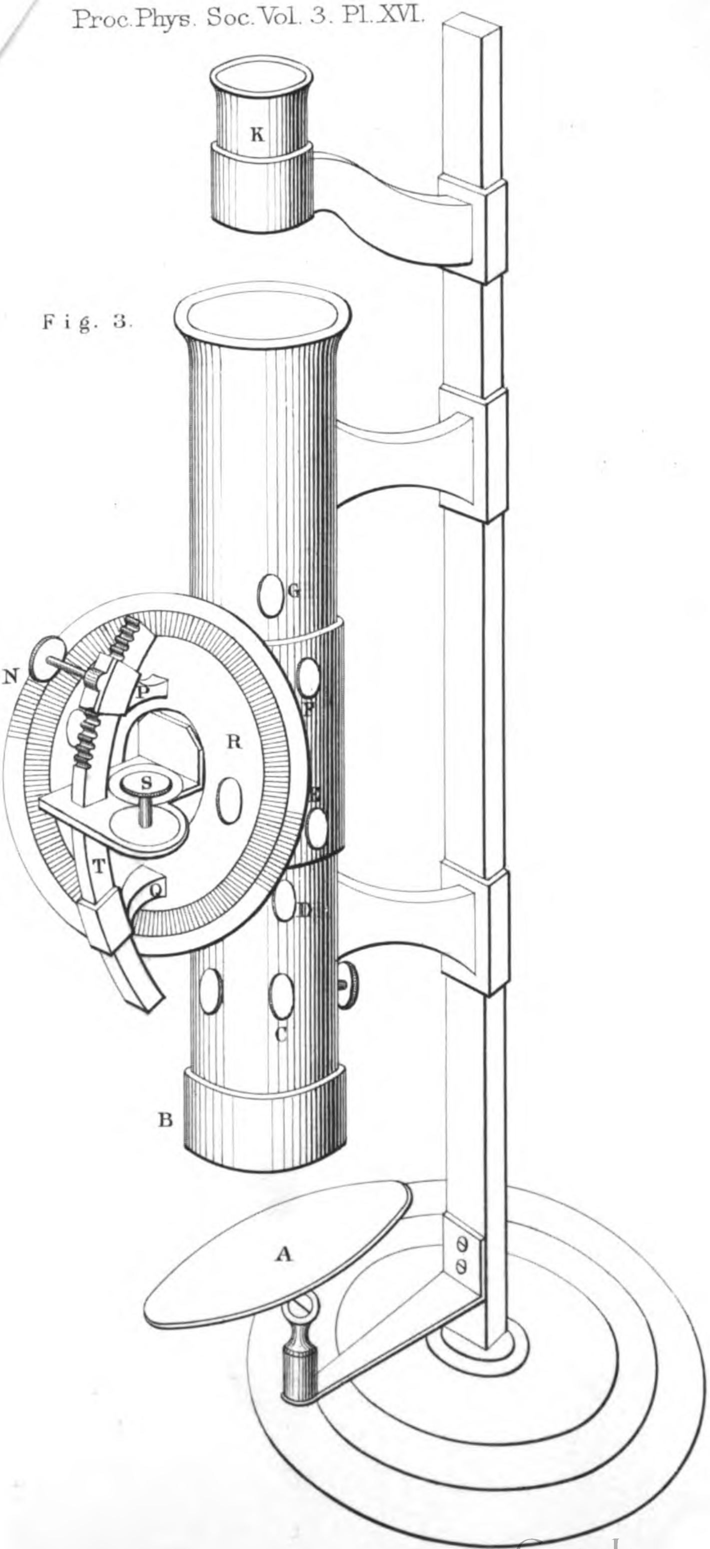
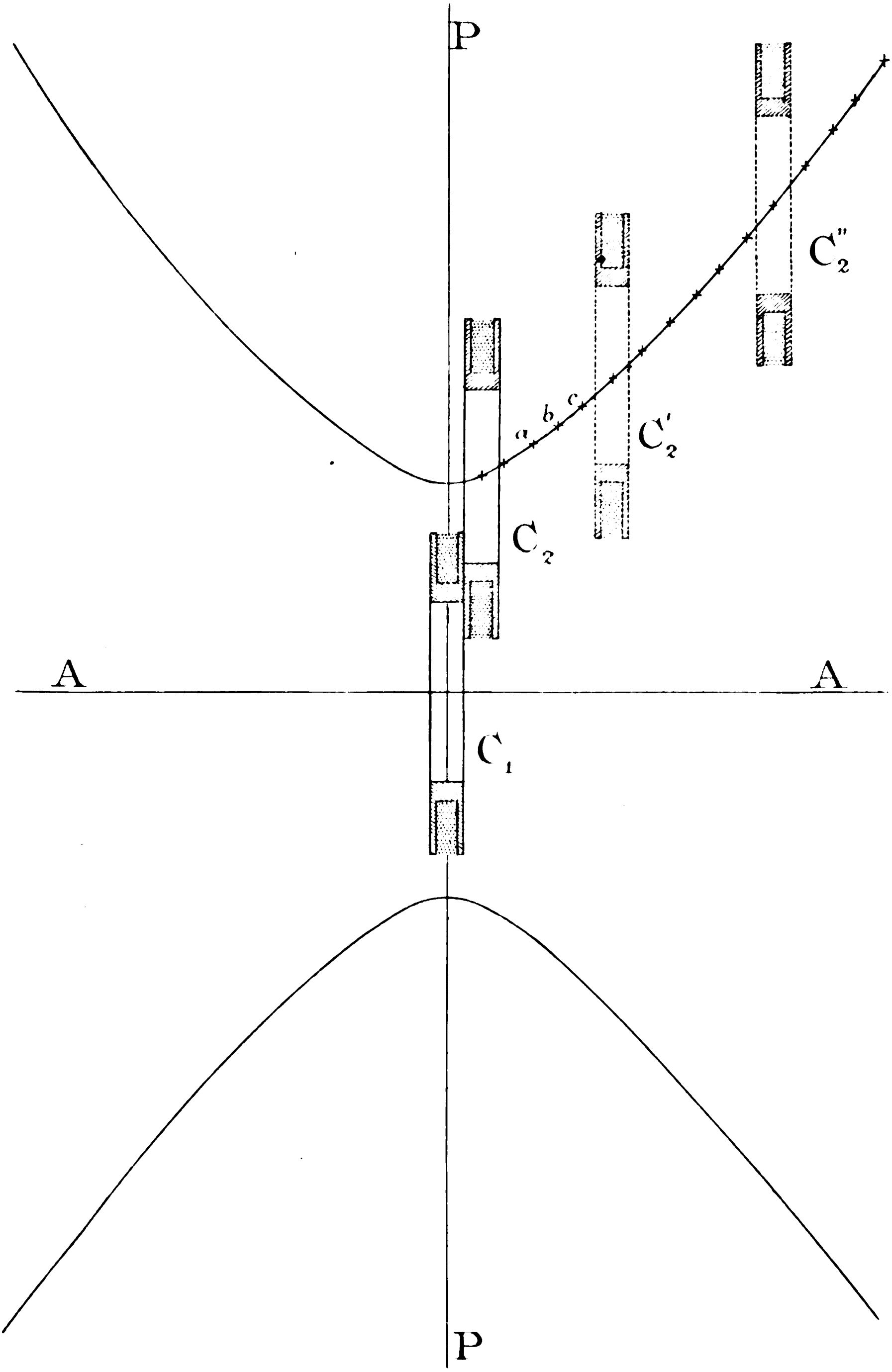


Fig. 3.





Mintern Bros lith

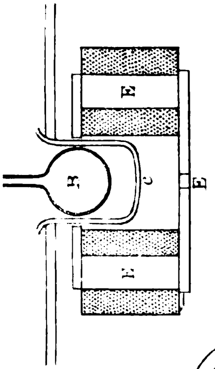


Fig 2.

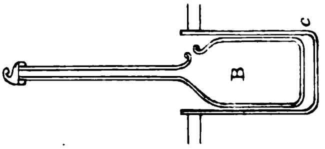


Fig 4.

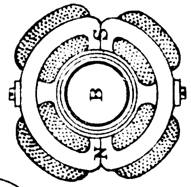


Fig. 3.

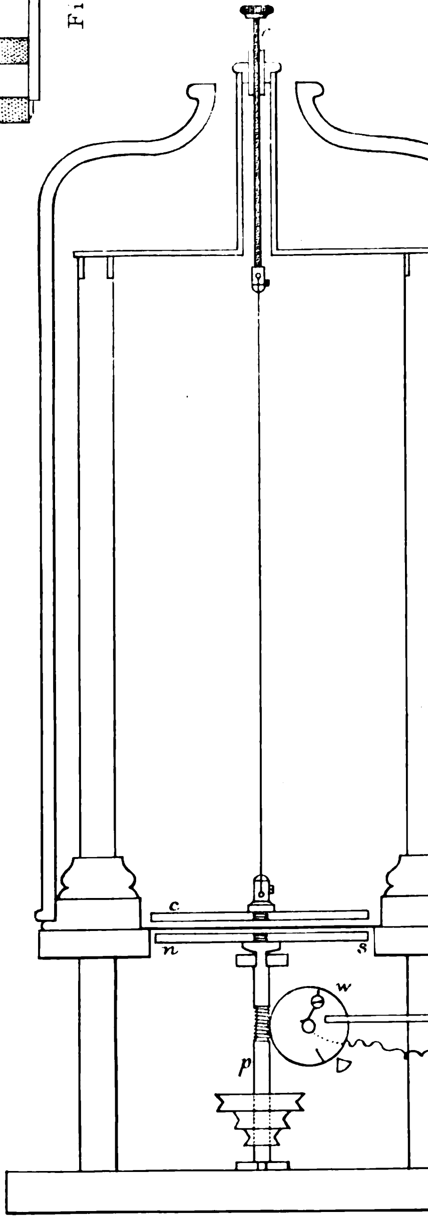
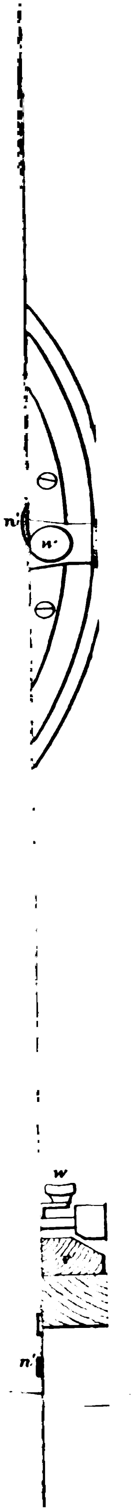


Fig. 1.

Scale $\frac{1}{3}$.

Mintern Bro^s lith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PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1878–79.

February 2nd, 1878.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The following were elected Members of the Society:—

M. T. **Cormack**, M.A.; C. J. **Faulkner**, M.A.; C.W. **Jones**, F.R.A.S.; C. **Leudesdorf**, M.A.; C. E. **Walduck**.

Prof. S. P. **Thompson** exhibited an experiment illustrating Ampère's law of parallel voltaic currents.

February 16th, 1878.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

G. **Herbert West**, M.A.

The following papers were read:—

"On Grove's Gas-Battery." H. F. **Morley**, B.A.

"On the Harmonograph." S. C. **Tisley**.

"On a modified form of Lippmann's Capillary Electrometer, and its employment for the detection of the Currents in Bell's Telephone." F. **Page**, B.Sc.

Exhibition of a Lantern-Galvanometer. Prof. S. P. **Thompson**.

March 2nd, 1878.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The following were elected Members of the Society:—

J. Parke Kirkman; Dr. W. J. **Russell**, F.R.S.

The following papers were read:—

"On the Coloured Figures caused by the Vibrations of Thin Films." **Sedley Taylor**, M.A.

"On Mr. Edison's Phonograph." W. A. **Preece**.

March 16th, 1878.

Special General Meeting.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The President of the Physical Society of Paris was elected an *ex officio* Member of the Society.

Ordinary Meeting.

The following were elected Members of the Society:—

J. S. **Bergheim**, C.E.; W. M. **Hicks**, M.A.; Dr. J. **Hopkinson**, C.E.; Miss **Edith Prance**; T. **Wills**.

The following papers were read:—

"On the Transmission of Vocal and other Sounds through Metallic Wires." W. J. **Millar**.

"On Thermoelectric Currents in Strained Wires." G. W. **von Tunzelmann**.

"On the Composition of Rectangular Vibrations." Prof. W. G. **Adams**, F.R.S.

"On the Passage through Colloids of the Products of Electrolysis." F. **Guthrie**, F.R.S.

March 30th, 1878.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The following were elected Members of the Society:—

Shelford Bidwell, M.A., LL.B.; **William Grant**; **Edmund Gurney**; **James Heseltine Smith**.

The following papers were read:—

"On Byrne's Pneumatic Battery." W. H. PREECE.

"On an Arrangement for obtaining Uniform Rotation." LORD RAYLEIGH, F.R.S.

April 13th, 1878.

Prof. R. B. CLIFTON, F.R.S., Vice-President, in the Chair.

The following were elected Members of the Society:—

Rev. T. NEVILLE HUTCHINSON, M.A.; Dr. B. W. RICHARDSON, F.R.S.; WILLIAM CAMPBELL; R. W. F. HARRISON.

The following papers were read:—

"On the Transmission of Vocal Sounds through Metallic Wires." Messrs. NIXON and HEAVISIDE.

"On the Photographs of the Less-refrangible end of the Spectrum obtained by means of Diffraction-gratings." Capt. ABNEY, C.E., F.R.S.

"On the Influence of Heat on the Transpiration of Gases." Prof. FRANK GUTHRIE.

Exhibition of Models of Crystals. Mr. BAUERMANN.

May 11th, 1878.

Prof. W. G. ADAMS, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

PHILIP MAGNUS, B.A., B.Sc.

The following papers were read:—

"On Recent Advances in the Chemistry of the Sun." J. NORMAN LOCKYER, F.R.S.

"On the effect of Stress on the Magnetization of Iron, Nickel, and Cobalt." Sir WILLIAM THOMSON, F.R.S.

May 25th, 1878.

Prof. W. G. ADAMS, F.R.S., President, in the Chair.

The following were elected Members of the Society:—

WILLIAM ROBERT PHELIPS; GEORGE MALTHUS WHIPPLE, B.Sc., F.R.A.S.; THOMAS MCENIRY; W. KIESER.

The following papers were read :—

"On Brass Wind Instruments as Resonators." J. N. Blaikley.

"On the effect of Torsion on the Electrical Conductivity of Brass." Sir William Thomson, F.R.S.

Exhibition of Photograph of Solar Spectrum taken by means of a Diffraction-grating. Dr. Rutherford.

June 8th, 1878.

Prof. W. G. Adams, F.R.S., President, in the Chair.

The following was elected a Member of the Society :—

Richard H. Solly, F.G.S.

The following papers were read :—

"On the Microphone." Prof. Hughes.

"On the Reflection of Light from the surface of Potassium Permanganate." Sir John Conroy, Bart., M.A.

"Electrical Properties of Bees'-wax and Lead-Chloride." Prof. W. E. Ayrton.

Exhibition of an inexpensive form of Optical Bench. Prof. S. P. Thompson.

June 22nd, 1878.

Prof. W. G. Adams, F.R.S., President, in the Chair.

The following was elected a Member of the Society :—

F. W. Grierson.

The following papers were read :—

"On a new Polariscope." The President.

"On Starch and Unannealed Glass under the Polariscope." Walter Baily, M.A.

"On the Flow of Water from Orifices at different Temperatures." Prof. W. C. Unwin.

"On Complementary Colours." Dr. Gorham.

"On Magnetic Figures illustrating Electrodynamic Relations." Prof. S. P. Thompson.

"On the Coordination of Space." C. H. Hinton.

November 9th, 1878.

Prof. W. G. Adams, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

Sir Frederick Elliot.

The following papers were read:—

"On the Interference of Diffused Rays of Light by Thick Plates." The President.

"On Mr. Edison's Tasimeter and Carbon Telephone." Prof. W. F. Barrett.

Exhibition of some Electric Lamps without Clockwork. William Ladd.

November 23rd, 1878.

Prof. W. G. Adams, F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

The Rev. P. R. Sleeman, F.R.A.S.

The following papers were read.:—

"On the Music of Colour and Visible Motion." Profs. Ayrton and Perry.

"On the Adjustment of a Collimator for Rays of different Refrangibilities." Dr. A. Schuster.

December 14th, 1878.

Prof. W. G. Adams, F.R.S., President, and afterwards Prof. G. C. Foster, F.R.S., Vice-President, in the Chair.

The following were elected Members of the Society:—

William Gleed, B.A.; James Gordon McGregor, M.A., D.Sc.

The following papers were read:—

"On a Condenser of Variable Capacity." C. V. Boys.

"On a Differential Water-vapour Thermometer." Dr. O. J. Lodge.

"On the Surface-tension of Liquid Sulphurous Anhydride." J. W. Clark.

January 25th, 1879.

Prof. G. C. **Foster**, Vice-President, in the Chair.

The following were elected Members of the Society:—

Alexander Macdonell, B.A.; E. **Ray Lankester**, F.R.S.

The following papers were read:—

"On a Constant Bichromate-of-Potash Battery." D. **Erck.**

"On the Vibration of Metallic Rods." F. **Guthrie.**

Exhibition of Commutators and Resistance-boxes by W. **Kieser.**

Annual General Meeting.

February 8th, 1879.

Prof. W. G. **Adams**, F.R.S., President, in the Chair.

The following Report of the Council was read by the President:—

The position and prospects of the Society continue to give the Council unmixed satisfaction, and its usefulness is steadily increasing.

The number of Members has risen from 232 at the date of the last Annual Meeting to 270 at the present time, and of the 38 new Members 16 have compounded. The value and interest of the papers, which have been more numerous than in any previous year, have been well sustained.

The Council regret that the Society has lost by death a Member, Capt. R. G. Scott.

Captain R. G. **Scott**, R.E., son of the Rev. Dr. Scott, of the Methodist College, Belfast, was educated principally in Ireland. He obtained his Commission in the Royal Engineers on the 25th of January, 1862. During service in the Mauritius he suffered severely from fever, one attack nearly proving fatal. He returned to England with a constitution seriously impaired; but this, however, did not prevent his studying Electricity with great ardour. His capacity for work and talent for organization enabled him to render important public service in connexion with experiments at Portsmouth on the use of large charges of gun-cotton in submarine mines; and his zeal and ability being recognized by his official superiors, he was appointed an Assistant Instructor in Telegraphy at the School of Military Engineering at Chatham, where his skill in Mathematics in its applications to Electricity was of much value. He died, March 16th, 1878, at the age of 35, too young to have made his name widely known; but his geniality, wit, and musical talent will be long remembered in his own circle of friends.

In accordance with the intimation contained in last year's Report, an opportunity was afforded to each of the Members of expressing an opinion as to the desirability of changing the time of meeting. Replies were received to the circular issued by the Council from 186 out of the 232 Members; and although the change was strongly urged by some, the Council did not consider that sufficient support was given to the proposal to justify them in altering the present arrangement.

The Library continues to progress satisfactorily. The following periodical works are regularly received, and are available for the use of Members:—the 'Journal de Physique,' Wiedemann's 'Annalen,' and the 'Beiblätter.' It has also been decided to purchase a complete set of 'Carl's Repertorium,' which will form a valuable addition to our collection. During the past year several foreign and other publications have been received, and the thanks of the Society have been given to the respective Donors. Among these the following may be mentioned:—A volume of the 'Annuaire du Bureau des Longitudes,' presented by our distinguished Honorary Member M. J. Janssen, which contains a review, written by himself, "On recent progress in Solar Physics;" Bibliographie Analytique &c., J. Plateau; Saunier's Horology, E. Rigg; University of Melbourne, Calendar and Exam. papers; Journ. Soc. Telegraph Engineers; Meteorology of Bombay Presidency, from the India Office; Connecticut Academy Transactions; Fourier's Theory of Heat (Freeman), Pitt Press; numerous papers from the Royal Society of New South Wales.

The Council have availed themselves of the power conferred on them by the amended Bye-Laws to recommend, as an ex-officio Member of the Society, the President of the Physical Society of Paris, and they are gratified at being able to congratulate the Members on the connexion which has been thus established with this distinguished Foreign Body. The Members of the Society will be gratified to hear of the satisfaction with which the announcement was received by the Members of the Physical Society of Paris. The President of the French Physical Society writes:—"Cette nouvelle a été accueillie avec la plus vive satisfaction, et l'assemblée a immédiatement voté à l'unanimité que le President de la

Société de Physique de Londres serait inscrit à perpétuité parmi les Membres de la Société Française de Physique. Nous éspérons que les liens qui vont unir les deux Sociétés se fortifieront de jour en jour, et nous sommes personellement heureux d'être chargés par nos confrères de vous adresser à vous et aux Physiciens de Londres les premières paroles de bonne confraternité."

The Council feel that no observations are necessary from them as to the illustrious Physicists whose names are submitted to you to day for election as Honorary Members, as it will be evident that their union with the Society cannot fail to add to its dignity.

The Society's Reprint of Sir Charles Wheatstone's published scientific papers is nearly complete. All the papers of which the existence is known to the Editors are in type, and, with the exception of the last two sheets, have been corrected and returned for press. The collected papers will form a volume of about 370 pages, which the Council confidently hope to place in the hands of Members in a few weeks.

The question of reprinting important Physical papers, to which allusion was made in last year's Report, has not been lost sight of; but owing to the expenses incurred for registration and for the reprint of Sir C. Wheatstone's papers, it has been found necessary to defer active steps until the current year.

The Council again regret that so few of the Members communicate papers to the Society: during the past year, however, the proportion of authors to papers read has been greater than in the previous year, and in the future it may perhaps be found useful for the Society to indicate definite subjects for research. In this connexion the Council would point to the words of Dr. Spratt, who, writing soon after the foundation of the Royal Society, which in organization our own so much resembles, observes, " That though the Society is not yet four years' old, and has been of necessity hitherto chiefly taken up about preparatory affairs, yet even in this time . . . . they have had successes in the trial of many remarkable things;" and he suggests " that by laying down on what course of discovery they intend to proceed, the gentlemen of the Society may be more solemnly engaged to prosecute the same, as they will not be able handsomely to draw back and to forsake such honourable intentions."

Our grateful thanks are due to the Lords of the Committee of Council on Education for their continued permission to hold our Meetings in the Science Schools, South Kensington.

At the time of our Annual Meeting last year we were only just beginning to recover from the excitement which was created by the invention of the Telephone, an instrument which, quite apart from the uses to which it may be applied, is of great interest to scientific men, and would have delighted Faraday, since it gives, by means of magneto-electricity, a very complete illustration of the laws of conservation, transformation, and dissipation of energy.

The year had scarcely began when we were startled at the fertility of resource of a far-famed inventor, who sends to us a scroll on which the very tones of his voice are engraved, and which may yet be heard over and over again and handed down to future ages.

The invention of the Phonograph gave rise to investigations on the Analysis of Vowel-Sounds and Resonance-Cavities by Prof. Fleeming Jenkin and others, which bear out the conclusions drawn by Prof. Willis and by Wheatstone from their experiments, that each vowel-quality is given by a particular resonator, and that the pitch of maximum resonance of the resonator is an important element in determining the vowel character of the sounds produced. They also support the view of Prof. Donders, that there is a pitch of maximum resonance in human mouth-cavities for the vowel *ö*, but give no such characteristic tone for the vowel *ü*. And they confirm the statement of Helmholtz that vowel-sounds are made up of harmonic partial tones.

The subject of the quality of sounds and resonance has been illustrated before the Society by Mr. Blaikley, who gave us an interesting paper, "On Brass Wind Instruments as Resonators."

The discovery of the Telephone led people to think of ordinary mechanical methods of transmitting vibrations, which were investigated by Wheatstone forty or fifty years ago, and two interesting papers on the transmission of Vocal and other Sounds through Metallic Wires, by Mr. Millar and by Messrs. Nixon and Heaviside, have been read before the Society.

In the Telephone, the voice reproduced at the receiving end must, from the dissipation of energy, be fainter than the human voice, which is the motive power of the apparatus. In the Microphone of Prof. Hughes, and in the Carbon Telephone of Mr. Edison, a transmitter of pieces of carbon or other substances in contact is employed to modulate the intensity of a battery current; and the tread of a fly, or the scratching of a single hair, or even the diffusion of a gas through a porous substance, may give rise to vibrations which act as a battery key to regulate and modify the flow of the current, so as to cause it to reproduce the same vibrations in the telephone plate, which is used as a receiving instrument.

In his Tasimeter, Mr. Edison has shown that the same combination of pieces of carbon may be employed to measure slight alterations of pressure, and so to measure slight changes of temperature by the expansion of a strip of ebonite or other substance, which presses on the carbon disks and alters the resistance of the electric circuit.

Prof. Hughes has shown that a Microphone may be used as a relay in an extended Telephone circuit, and also that messages may pass opposite ways through the Microphone without interfering with one another. It has also been shown that this instrument may be of service in surgical operations, and that it may possibly be of service in enabling the deaf to hear.

In connexion with this subject, as well as with the ordinary methods of transmitting sound-vibrations, I may quote the words of Hooke, who was Physical Demonstrator to the Royal Society rather more than 200 years ago.

Hooke, in the Preface to his 'Micrographia,' published in 1665, says:—"As glasses have highly improved our seeing, so, t'is not improbable, but that there may be found mechanical inventions to improve our other senses of hearing, smelling, tasting, touching. T'is not impossible to hear a whisper a furlong's distance, it having been already done; and perhaps the nature of the thing would not make it more impossible though the furlong should be ten times multiply'd. And though some famous authors have affirmed it impossible to hear through the thinnest plate of Muscovy glass; yet I know a way by which t'is easie enough to hear one speak through a wall a yard thick. It has not yet been thoroughly examined how far Otocousticons may be improved, nor what other wayes there may be of quickening our hearing, or conveying sound through other bodies then the air; for that that is not the only medium I can assure the reader that I have by the help of a distended wire propagated the sound to a very considerable distance in an instant, or with as seemingly quick a motion as that of light, at least incomparably swifter then that, which at the same time was propagated through the air: and this not only in a straight line, or direct, but in one bended in many angles."

These remarkable inventions, which one after another have created so much excitement, and which have even sent a thrill through the most learned of Societies, have been well illustrated at our Meetings.

At the same time the list of papers read before the Society shows that we have had important communications on all branches of Physics. In Electrical science we have had suggestive papers from Sir William Thomson on the relation between Stress and Magnetization, and again, on the relation between Stress and Electrical Conductivity; by Mr. Tunzelmann on Straining of Wires and Thermo-electric currents; and from Prof. Ayrton on Specific Inductive Capacity. Among our communications on the Interference and Polarization of Light, we have had a very carefully worked out paper by Mr. Walter Baily, "On Starch and Unannealed Glass under the Polariscope." Bearing on that branch of science with which the name of M. Plateau is associated, in which we make some attempt to approach the workings of molecules, we have had a paper from Prof. Unwin, and a paper on the Surface-tension of Liquids from Mr. Clark; and we shall none of us be in danger of forgetting the beauty of the coloured figures caused by the vibrations of Mr. Sedley Taylor's soap-films.

There is also an appropriateness in connecting the name of M. Kirchhoff with our Society, for, as regards Spectrum Analysis, we have been highly favoured: we have had a paper by Mr. Lockyer, "On Recent Advances in the Chemistry of the Sun," and have seen a new and wide field of research opened out by Captain Abney's success in photographing the less-refrangible end of the spectrum. We have also had the honour of welcoming amongst us Mr. Rutherford from America, who exhibited to the Society the remarkable photograph of the solar spectrum which he has taken, and to whom we are indebted for the production of the most perfect diffraction-gratings which the world has ever seen.

We thus see, from a hasty glance at the papers which have come before us during the past year, that something has been gathered from each of the many branches into which Physics is divided.

There is still one subject, which is by no means new, but to which I should draw your attention in reviewing what has been done during the past year. Through the kindness of Dr. Siemens, Mr. Latimer Clark, Prof. Tyndall, and Mr. Spottiswoode, I am able to-day to show you the results of various applications of the electric current for the production of light. A communication on this subject will be made to the Society by Mr. Shoolbred after the conclusion of the business of the Annual General Meeting.

The Society then proceeded to the election of Council and Officers for the ensuing year, and the following gentlemen were declared duly elected:—

President.—Prof. W. G. ADAMS, M.A., F.R.S.

Vice-President (who has filled the Office of President).—Prof. G. C. FOSTER, F.R.S.

Vice-Presidents.—Prof. R. B. CLIFTON, M.A., F.R.S.; Lord RAYLEIGH, M.A., F.R.S.; W. SPOTTISWOODE, LL.D., Pres. R.S.; Sir WILLIAM THOMSON, LL.D., D.C.L., F.R.S.

Secretaries.—Prof. A. W. REINOLD, M.A.; W. CHANDLER ROBERTS, F.R.S.

Treasurer.—Dr. E. ATKINSON.

Demonstrator.—Prof. F. GUTHRIE, Ph.D., F.R.S.

Other Members of Council.—Capt. W. De W. ABNEY, R.E., F.R.S.; WARREN DE LA RUE, D.C.L., F.R.S.; Major E. R. FESTING, R.E.; Prof. F. FULLER, M.A.; W. HUGGINS, D.C.L., F.R.S.; Prof. A. B. W. KENNEDY, C.E.; Prof. H. M'LEOD; The EARL OF ROSSE, D.C.L., F.R.S.; G. JOHNSTONE STONEY, M.A., F.R.S.; R. WORMELL, D.Sc.

After the names of the Officers and Council had been announced from the Chair, votes of thanks were proposed to the Lords of the Committee of Council on Education, to the President, to the Treasurer, Demonstrator, the Secretaries, and to the Auditors.

The Treasurer in Account with the Physical Society, from December 31st, 1877, to December 31st, 1878.

Dr.	£	s.	d.	£	s.	d.
Balance in Bank December 31st				154	8	4
Entrance-Fees				38	0	0
Subscriptions for 1876				5	0	0
" 1877				17	0	0
" 1878				120	0	0
" 1879				4	0	0
Compositions				160	0	0
Dividends on £400 4 per cent. Furness Debenture Stock, less Income Tax, 4*s.* 8*d.*				15	15	4
Dividends on £300 5 per cent. Midland Preference Stock, less Income Tax, 4*s.* 4*d.*				14	15	8
Sales of Everett's C.G.S.				11	2	4
Subscription paid to Taylor and Francis				1	1	0
				£541	2	8
To Balance				4	15	2

Cr.	£	s.	d.	£	s.	d.
Balance due to Treasurer				6	10	3
Purchase of £160 5 per cent. Midland Preference Stock				198	18	3
Reports of Meetings				13	7	3
Messrs. Miller and Wiggins for expenses of Registration				70	16	9
Messrs. Taylor and Francis:—						
Printing Proceedings &c., part iv. vol. ii.				23	17	6
Postage and addressing				1	19	6
Separate copies for Members				4	14	6
Miscellaneous Printing				21	18	6
Commission on sale of Everett's C.G.S.				1	2	4
Reprint of Sir C. Wheatstone's papers				110	16	0
Mr. Richardson, of Greenwich:—						
Miscellaneous Printing				1	11	6
Mr. Webb: Printing				0	19	6
Messrs. Smith and Son: Advertising				3	19	6
Mr. Clarke: Engraving				3	2	3
Periodicals				5	8	0
Stationery				1	17	10
Petty Cash:—						
Mr. Roberts		16	6			
Mr. Reinold	1	15	10			
Dr. Atkinson	2	13	6			
				5	5	10
Chapman: Attendance at Meetings				4	12	9
Cash in Bank				55	9	6
Balance due by Treasurer				4	15	2
				£541	2	8

We have examined the above Account, which we find correct, and that there is a Balance due by the Treasurer of Four Pounds, Fifteen Shillings, and Two Pence.

London, February 7th, 1879.

B. WOODD SMITH, } *Auditors.*
ROBT. J. LECKY. }

PROPERTY ACCOUNT. PHYSICAL SOCIETY.

Assets.	£	s.	d.	Liabilities.	£	s.	d.
Subscriptions due	40	0	0	Wheatstone's Reprint	90	0	0
£400 4 per cent. Debenture Stock Furness Railway at 103	412	0	0	Subscriptions in advance	4	0	0
£460 5 per cent. Preference Stock Midland Railway at 123	565	0	0	Due for Everett's C.G.S.	15	0	0
Due by Treasurer	4	15	2	Balance	968	4	8
Balance in Bank	55	9	6				
	£1077	4	8		£1077	4	8

We have examined the above Account, which we find correct, and that the Assets of the Society exceed the Liabilities by Nine Hundred and Sixty-eight Pounds, Four Shillings, and Eight Pence. We have also examined the Securities at the Bankers.

London, February 7th, 1879.

B. WOODD SMITH,
ROBT. J. LECKY. } *Auditors.*

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY OF LONDON.

SESSION 1879-80.

February 8th, 1879.

Prof. W. G. Adams, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

W. N. Shaw, M.A.; G. B. Finch.

The following papers were read:—

"On the Variation with Temperature of the Thermal Conductivity of a bar heated at one end." By O. J. Lodge, D.Sc.

"On Electric Lighting, Dynamo-Electric Machines, and Electric Lamps." By J. N. Shoolbred, C.E.

February 22nd, 1879.

Prof. W G. Adams, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

Prof. J. C. Adams, M.A., D.C.L., F.R.S.; Prof. G. D. Liveing, M.A.; F. W. Peterson; Rev. Coutts Trotter, M.A.

The following papers were read:—

"On an Electric Current Regulator." By Dr. C. W. Siemens, F.R.S

"On Spectra of Lightning." By Dr. A. Schuster.

"Some remarks on Dr. Lodge's paper 'On the Variation with Temperature of the Thermal Conductivity of a Bar.'" By Prof. W. E. **Ayrton.**

March 8th, 1879.

Prof. W. G. **Adams**, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

Dr. T. **Archer Hirst**, F.R.S.; F. **Jacob.**

The following papers were read:—

"On a New Theory of Terrestrial Magnetism." By Profs. W. E. **Ayrton** and J. **Perry.**

"On an Anomalous Experiment with the Quadrant Electrometer." By Dr. J. **Hopkinson**, F.R.S.

"On the Maintenance of Constant Temperatures and Pressures." By F. D. **Brown.**

March 22nd, 1879.

Prof. W. G. **Adams**, M.A., F.R.S., President, in the Chair

The following was elected a Member of the Society:—

Lieut. **Hastings** R. **Lees**, R.N.

The following papers were read:—

"On the Photographic Method of Registering Absorption-Spectra." By Capt. **Abney**, F.R.S.

"On the Fracture of Colloids." By Dr. F. **Guthrie**, F.R.S.

April 26th, 1879.

Prof. W. G. **Adams**, M.A., F.R.S., President, in the Chair.

The following papers were read:—

"On some Phenomena connected with Magneto-electric Induction." By Dr. F. **Guthrie**, F.R.S., and C. V. **Boys.**

The following Notes from the Physical Laboratory of University College, Bristol. By Prof. S. P. **Thompson**:—

1. On the Source of Sound in the Bell Telephone.
2. On a new variety of Magnetic Figures.

3. On Magnetic Figures for demonstration.
4. On the Magnetic behaviour of fixed Iron-filings.
5. Magnetic Figures of Three Dimensions.

May 10th, 1879.

Prof. W. G. ADAMS, M.A., F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

JOHN CASTELL EVANS.

The following papers were read:—

"On an Inquiry into the suppression of the Induction-disturbance of the Telephone." By Prof. W. F. BARRETT.

An Account and Exhibition of Gower's Loud-speaking Telephone. By Mr. WOLLASTON.

"On the Divisibility of the Electric Light." By W. J. WILSON.

"On a new form of Incandescent Electric Lamp for Surgical Purposes." By W. H. COFFIN.

May 24th, 1879.

Prof. W. G. ADAMS, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

WALTER EMMOTT; SEDLEY TAYLOR, M.A.

The following papers were read:—

"On a New Harmonograph." By W. J. WILSON.

"On a New Induction-Balance." By Prof. D. E. HUGHES.

"On the Examination of certain Alloys by the Aid of the Induction-Balance." By W. C. ROBERTS, F.R.S.

"On a New Pump without Valves." By Dr. ERCK.

June 14th, 1879.

Prof. W. G. ADAMS, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

DONALD M'ALISTER, M.A.; ST. GEORGE LANE FOX.

The following papers were read:—

"On the Suppression of the Induction-disturbance in a Telephone Circuit." By Prof. H. M'LEOD.

"On a New Reversing Key for Electrometer Work." By Dr. O. J. Lodge.

"On the Sensitiveness of Electrical Discharges in Vacuo." By W. Spottiswoode, F.R.S., and J. F. Moulton, M.A.

Dr. Henry Draper gave an account of his observations on the bright lines of oxygen in the Solar Spectrum.

Specimens of Chromo-photographs by Dr. J. Albert of Munich were exhibited.

June 21st, 1879.

Extra Meeting, held at the Royal Indian Engineering College, Cooper's Hill.

Lord Rosse, F.R.S., in the Chair.

The following papers were read:—

"Experiments on the Friction of Water." By Prof. W. C. Unwin.

"On a Method of determining the Vibration-number of Tuning-Forks." By Lieut. G. S. Clarke, R.E.

"On an Electrical Clock." By Prof. M'Leod.

"On the Surface-tension of Liquids at Temperatures near their Critical Points." By J. W. Clark.

June 28th, 1879.

Prof. W. G. Adams, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

J. J. Eastick; J. Fletcher Moulton, M.A.

The following papers were read:—

"On a new Measuring-Polariscope." By Prof. W. G. Adams, M.A., F.R.S.

"On the Distribution of Heat in the Solar and other Spectra." By Sir John Conroy, Bart., M.A.

"On the Conjugate Positions of two Circular Coils." By W. Grant.

"On the Spirality of the Energy of a Magnet." By Dr. R. C. Shettle.

"Remarks on Profs. Ayrton and Perry's Theory of Terrestrial Magnetism." By Prof. Rowland, of Baltimore.

"On a modification of Arago's experiment." By Walter Baily, M.A.

"On a form of Galvanometer for showing the internal resistance of a battery." By Conrad Cooke.

November 8th, 1879.

Prof. W. G. Adams, M.A., F.R.S., President, in the Chair.

The following papers were read:—

"On the Analogy between the Heat-conductivity and the Induction-Balance-effect of Tin-copper Alloys." By W. Chandler Roberts, F.R.S.

"On the Electrical Conductivity of the Alloys examined by Mr. Roberts." By Dr. O. J. Lodge.

"On a Standard Galvanic Cell." By Capt. Armstrong, R.E.

"On the Incandescence of Electrodes, an effect observed by Prof. Pirani, of Melbourne." By Prof. F. Guthrie, F.R.S.

November 22nd, 1879.

Prof. W. G. Adams, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

Prof. Calcott Reilly, M. Inst. C.E.;
Prof. A. H. Heath, Assoc. Inst. C.E.

The following papers were read:—

"On a Retention-image Photometer." By Prof. F. Guthrie, F.R.S.

"On a Suggestion as to the Molecular constitution of Chlorine furnished by the Dynamical theory of Gases." By Prof. A. W. Rücker.

"On the effect of Temperature on certain forms of Induction-coils, with especial reference to the physiological effects in the human body." By Dr. R. C. Shettle.

Crossley's Telephone Transmitter was exhibited by Mr. Walter Emmott.

December 13th, 1879.

Prof. W. G. ADAMS, M.A., F.R.S., President, in the Chair.

The following were elected Members of the Society:—

HENRY RICHMOND DROOP, M.A.; RICHARD TETLEY GLAZEBROOK, M.A.; E. PATERSON; JOHN HENRY POYNTING, M.A.; Prof. H. A. ROWLAND; E. B. SARGENT, B.A.; R. C. SHETTLE, M.D.; JOHN GRAY, B.Sc.

The following papers were read:—

"On a new Wheatstone's Balance for comparing Standard Coils." By Dr. J. A. FLEMING.

"On a Dispersion-Photometer." By Profs. AYRTON and PERRY.

"On the value of 'g' in Japan."

A new form of Spherometer, designed by Mr. WALTER GOOLDEN and constructed by A. HILGER, was exhibited and described.

January 24th, 1880.

Prof. W. G. ADAMS, M.A., F.R.S., President, in the Chair.

The following was elected a Member of the Society:—

W. E. VINSON.

The following papers were read:—

"On Induction in Telephonic Circuits." By W. GRANT.

"On Intermittent Currents, and the theory of the Induction-Balance." By Dr. O. J. LODGE.

"On a Liquid Voltaic Arc." By C. V. BOYS.

A Talking-machine was exhibited and described by its constructor, Mr. F. FABER.

Annual General Meeting.

February 14th, 1880.

Prof. ADAMS, M.A., F.R.S., President, in the Chair.

The following Report of the Council was then read by the President:—

Those who have interested themselves in the welfare of our

Society cannot fail to have been gratified by the progress that has been made during the past Session. The papers submitted to the Society, which are more numerous than in any previous session, have for the most part been prepared by the respective authors for publication. This fact has given the Council much satisfaction, as it has enabled the Publication Committee to issue the 'Proceedings' with greater promptitude than heretofore. While the Council hopes that Members will, as far as possible, provide written papers and abstracts, it is anxious to encourage the exhibition of the simplest new experiment at the Meetings, or the informal communication of any results of a novel and interesting character.

Last year *i.e.* from February 1878 to February 1879, fifteen papers were printed in our Proceedings. This year there were fourteen papers already published in October last, and during the past week another number of the Proceedings has been published containing ten papers, making twenty-four papers during the year, and including papers which have been read up to our last Meeting.

The Society is to be congratulated on having two energetic and efficient Secretaries, who have during the past year brought the publication so closely up to date that the number of Proceedings containing a valuable paper read at our last Meeting on January 24 is already in the hands of Members before the present Meeting of the Society.

The Society is also to be congratulated on the increase in the number of papers considered worthy of publication.

A study of our Proceedings will show that in these days of hasty printing they do not encumber, and thereby obstruct, the progress of Science, but that they form a valuable contribution to the literature of Physics.

On the day of our last Annual Meeting Mr. Shoolbred gave an account of the more recent development of electric lighting, illustrated by diagrams of the various systems in use; and at the next Meeting Dr. Siemens described his new electric current regulator, based on the heating of a wire by the passage of an electric current through it. This apparatus he devised to regulate the flow of a current so as to maintain it uniform, however the resistance of the circuit or the electromotive force may vary. Early in March Capt. Abney read an important paper on obtaining photographic records of absorption-spectra; and Prof. Guthrie gave the first results of an elaborate investigation into the fracture of colloids. In April, in conjunction with Mr. C. V. Boys, he described some experiments on the subject of Arago's rotation, in the course of which they observed that circular cuts in the disk are more important than radial cuts; and by extending their research to liquids they have arrived at an interesting method of determining the conductivities of liquids, in which the errors due to polarization are eliminated.

At our Meeting in May, Prof. Hughes exhibited his induction-balance and sonometer, which promise to play so important a part in physical research; and none can fail to recognize the care, skill, and untiring patience that must have been devoted to experiments that have resulted in these remarkable instruments. The beautiful research on the stratified discharge in vacuum tubes, by Messrs. Spottiswoode and Moulton, was illustrated before the Society by the latter physicist; and on the same day we were favoured by a communication from Dr. H. Draper on the presence of oxygen in the sun.

A very interesting extra Meeting of the Society was held on the 21st of June at the Royal Engineering College, Cooper's Hill, by the kindness of Col. Chesney, when papers were read by Profs. Unwin and McLeod, Lieut. Clarke, and Mr. J. W. Clark, all members of the College staff.

A new theory of the earth's magnetism, put forward by Messrs. Ayrton and Perry in the earlier part of the session, was based on the results of experiments made by Prof. Rowland, of Baltimore; and we were fortunate, at our concluding meeting of last Session, in having Prof. Rowland himself with us, who gave an account of his experiments on which the theory was based, and his views as to how far it is tenable.

An interesting investigation in the dynamical theory of gases has afforded Prof. Rücker a suggestion as to the constitution of chlorine, and the sonometer and induction-balance have laid open a rich field of physico-mathematical research, which has been taken advantage of by Mr. Poynting, who studied the graduation of the former instrument, and by Dr. Lodge as regards the induction-balance and the general theory of intermittent currents. Mr. Roberts has shown experimentally that in the case of the copper-tin alloys the curve of the conductivity for heat corresponds closely with that representing the induction-balance effect, at the same time pointing to the incomplete character of Matthiessen's curve of electric conductivity. A paper on an allied subject, the induction in telephonic circuits, was read by Mr. Grant.

Prof. Pirani, of Melbourne, drew the attention of Dr. Guthrie to a remarkable effect that is produced when the terminals of a powerful battery are immersed in succession in various liquids, and Prof. Guthrie exhibited a number of experiments illustrative of the subject before the Society.

Several new instruments of research have been brought before us during the past year; among them may be mentioned Prof. Guthrie's image-retention photometer, a measuring polariscope by the President, and a valuable modification of the Wheatstone Bridge, devised by Dr. A. J. Fleming.

The Conversazione given by the President and Council on the 7th inst. was in every way successful. About 600 members and their friends were present. The Council looks forward to repeating such meeting from time to time.

The Library Committee reports that the Library consists now of 236 separate volumes and about 150 pamphlets.

Last summer an invitation was issued to Members to aid in the formation of the Library by contributing copies of their own researches and other works. In response to this, many valuable contributions have been received. Amongst these may be mentioned works by Descartes, Helmholtz, Tyndall, and Thomson, presented by J. Sterling; a very large collection of books and pamphlets on subjects relating to electricity, and having an especial historical interest, presented by Latimer Clark; Plateau's 'Statique Expérimentale et Théorique des Liquides,' presented by the author; the 'Observatory,' nos. 1–27, presented by W. H. M. Christie; about twenty volumes on miscellaneous scientific subjects, presented by J. B. Huntington; Janssen's 'Mélanges Scientifiques,' comprising nearly a complete set of his works, by the author. The following gentlemen have been also good enough to make very acceptable contributions:—W. Besant, M.A. (Whewell's 'History of the Inductive Sciences,' 3 vols.); Prof. B. Stewart (Treatise on Heat); Dr. W. M. Watts (Watts's 'Index of Spectra'); Rev. J. Perry ('Meteorological Observations, Stonyhurst'); R. H. M. Bosanquet, M.A. (Treatise on Temperament); Dr. W. Spottiswoode (Polarization of Light); F. D. Brown (Arnott's 'Natural Philosophy,' 2 vols.); E. Rigg, M.A. (Saunier's 'Treatise on Modern Horology').

The Council again propose to add two names, those of Clausius and Fizeau, to our list of illustrious Honorary Members, of whom it must be remembered that, in accordance with our Bye-laws, after this Meeting, only four will remain to be selected.

The claims of the distinguished men whose names are placed before you for election as Honorary Members are so well known that it is scarcely necessary that special attention should be drawn to them.

M. Hippolyte Louis Fizeau is known for his investigations, some forty years ago in connexion with M. Foucault, on the use of bromine in the preparation of Daguerreotype plates, and on the action of the red rays on sensitive substances, also for his researches on the Electric Arc, as well as on the Interference and Polarization of Light, and the Interference and the Wave lengths of Heat radiation. But M. Fizeau is more widely known as having been the first to determine (more than thirty years ago) the velocity of light by actual experiment on the earth's surface, by sending a beam of light to a mirror, 8633 metres off, and measuring, by means of revolving wheels, the time it takes to go and return to the station of the observer. Soon after M. Fizeau carried out the experiment which had been suggested by Arago years before, founded on Wheatstone's method of the revolving mirror, and presented his results to the Academy in June 1850. He had previously presented a note on his method on May 6 of the same year, the day on which M. Foucault presented his complete memoir on the subject to the Academy.

M. Rudolph Julius Emmanuel Clausius is well known for his investigations on the Mechanical Theory of Heat, and his work on the Kinetic Theory of Gases will now become more widely known to English readers through the translation which has lately been issued, with the sanction of Professor Clausius himself. It will perhaps be not too much to say that there is no one to whom we are more indebted for the creating anew and the complete development of this special branch of Science.

Our grateful thanks are, as heretofore, due to the Lords of the Committee of Council on Education for permission to hold our meetings in the Science Schools, and to Dr. Guthrie, who has continued to act as Demonstrator.

If we attempt to compare the progress made in Physics with the progress made in other branches of science, we shall find that not only will Physics stand well to the front, but that other Sciences are getting more and more interwoven with Physics, and are advancing, chiefly by means of the investigations and discoveries which Physics brings to their aid, and by reason of the mutual dependence of the various physical forces on one another.

The increasing demand for our 'Proceedings' by other scientific bodies would alone afford abundant proof of the appreciation in which the Society's work is held, even if the evidence afforded by its continued growth were wanting. The number of Members has risen from 266, at the date of our last Annual Meeting, to 298 at the present time. 17 Members have compounded during the year.

The Society has to lament the loss of three of its Members.

Mr. **Richard Secker Brough**, an original Member of our Society, was the youngest son of Thompson Brough, Esq., M.D. He was born at Killegan, in the county of Wicklow, on the 17th of October, 1846, and received his early education at a private school in Jersey, and later at the Victoria College. At Avranches he subsequently studied the Italian and German languages and Physical Science, and showed great interest in Mathematics.

It was about this time that Sir Stafford Northcote introduced the present system of examination for the Indian Telegraph Service. Mr. Brough was nominated for that service, and, after another year's study at Southampton and University College, London, he was appointed, by the Secretary of State for India, a fourth-grade Assistant Superintendent in 1869. His ability was so marked that he was appointed Assistant Superintendent of Stores in 1870. Shortly afterwards he was transferred to Madras to take charge of that important Telegraph Station. In March 1871 he was recalled to be appointed an Assistant to the Superintendent Electrician, which important position he held up to the time of his death. In the Electrician's office, where physical experiments of varied nature are daily executed, he had all the opportunity to develope his special talents, which he did with marked success. During Mr. Schwendler's absence from India, he was invariably deputed by the Director General to officiate for that officer, on account of the very great ability which he had shown in all the technical branches connected with telegraph administration. His latest practical work, in connexion with the Indian Telegraph, was undertaken with the special purpose of testing and repairing the faulty Paumben cables in connexion with the Ceylon cable.

Mr. Brough was the author of numerous papers on electrical subjects, which were published in the 'Proceedings of the Asiatic Society of Bengal.'

He was one of the most active members of the Asiatic Society, and since 1871 has been a member of the Physical Science Committee of that Society. He was elected an associate member of the Institution of Civil Engineers. He was also a member of the Society of Telegraph Engineers in England.

His kind and generous disposition was especially shown towards those who had the fortune to serve under him.

Mr. Le Neve Foster was perhaps the best known of those Members whom the Society has lost by death during the past year.

Mr. Foster, who died in February last, had been Secretary of the Society of Arts since 1853. He was born in 1809, and was the son of Mr. Peter Le Neve Foster, of Lenwade, Norfolk. He was educated under Valpy at Norwich, and from thence went up to Trinity Hall, Cambridge, where he was thirty-eighth Wrangler in the Mathematical Tripos of 1830, and was afterwards elected Fellow of his College. Called to the bar at the Middle Temple in 1836, he practised as a conveyancer until he became Secretary to the Society of Arts. He was intimately associated with all the earlier International Exhibitions, from 1851 downwards. Taking a general interest in many branches of Science, the subject of Photography was the one to which he principally devoted himself. He was one of the first to practice the art as a scientific amateur, and wrote a good deal on it, principally in the 'British Journal of Photography.' He was one of the founders of the Photographic Society, and served for many years on its Council. For twelve years he acted as Secretary of the Mechanical Section of the British Association, the last time being at the Meeting at Brighton in 1872. Mr. Foster's long connexion with the Society of Arts made his a well-known figure in all scientific circles; and it may certainly be said that wherever he was known he was greatly liked. He was not an original Member of the Society, but joined it in 1877, soon after its formation.

The next on our obituary list is one who was cut off before he had time to confirm the reputation which certainly would have been his in a very few years.

Mr. Thomas Wills was one of the most promising of our younger generation of chemists. He had not long been a Member of our Society, for it was only in March 1878 that he joined it. Most of his work lay in matters outside our range; though his recent work, in connexion with gas-lighting and of the products of combustion in the electric arc, was associated not less with physics than with chemistry.

Mr. Wills was born in 1850, and was educated at University College School and at King's College. In 1868 he became Assistant to Dr. Odling at Bartholomew's Hospital; and when Dr. Odling became Fullerian Professor at the Royal Institution, Wills accompanied him as Assistant. In 1873 he resigned his post to become Demonstrator in Chemistry at the Royal Naval College, Greenwich, and in the year following he became Secretary to the Chemical Section of the Society of Arts, on its foundation. He was Secretary for one year to the Chemical Section of the British Association, and was a Member of the Association Committee for ascertaining the best methods of utilizing the illuminating power of coal gas. He read many papers before the Chemical Society, the British Association, and the Society of Arts, and other Societies. For one of his papers he received a Society of Arts Medal, as he did also for a series of Lectures on Explosions in Coal-mines, given in 1878 before that Society.

Though Prof. CLERK MAXWELL was not a Member of the Society, the Council cannot refrain from recording their share in the profound regret that has been so widely expressed for his untimely death; for, in the graceful words of the President of the Royal Society, "in Maxwell the full maturity of a mind which had suffered no check or shadow of abatement, was devoted to the foundation of a more thorough knowledge of molecular physics and electrical laws than had hitherto been attained."

The publications other than the 'Proceedings,' which are intended to form an important feature of our work, still actively engage the attention of the Council. The first work published by the Society, 'The Centimetre-gramme-second System of Units,' by Prof. Everett, was found to be so useful that the entire edition was rapidly distributed; and, as a second edition was called for, the Council willingly acceded to the request of Prof. Everett, that his work, supplemented by a collection of physical data, should form one of the well-known series of mathematical treatises published by Messrs. Macmillan. A Dutch translation of the first edition appeared in 1877.

By the issue of the collected works of the late Sir Charles Wheatstone an important addition has been made to scientific literature, and the Members cannot fail to share in the appreciation with which it has been so generally received.

The Reprint Committee has decided to undertake the translation of the more important of Volta's papers on Electricity. They are supported in this decision by the opinions of Sir William

Thomson and Professor Clifton, who have given valuable advice as to the selection of papers for translation. Arrangements have been made for the translation of some of these papers under the supervision of the Committee, and the work will be taken in hand at once.

The Treasurer presented an audited Balance-sheet, and the Society then proceeded to the election of the Council and Officers for the ensuing year, and of Honorary Members. Mr. **Bidwell** and Mr. **Grant** were appointed Scrutators. The following were declared duly elected:—

President.—Sir **Wm. Thomson**, LL.D., D.C.L., F.R.S.

Vice-President (who has filled the Office of President).—Prof. W.G. **Adams**, M.A., F.R.S.

Vice-Presidents.—Prof. R. B. **Clifton**, M.A., F.R.S.; Lord **Rayleigh**, M.A., F.R.S.; W. **Spottiswoode**, LL.D., Pres.R.S.; W. W. **Huggins**, D.C.L., F.R.S.

Secretaries.—Prof. A. W. **Reinold**, M.A.; W. **Chandler Roberts**, F.R.S.

Treasurer.—Dr. E. **Atkinson**.

Demonstrator.—Prof. F. **Guthrie**, Ph.D., F.R.S.

Other Members of Council.—Capt. W. De W. **Abney**, R.E., F.R.S.; **Walter Baily**, M.A.; J. H. **Cotterill**, M.A., F.R.S.; **Warren De La Rue**, D.C.L., F.R.S.; Major E. R. **Festing**, R.E.; Prof. G. C. **Foster**, F.R.S.; Prof. F. **Fuller**, M.A.; J. **Hopkinson**, D.Sc., M.A., F.R.S.; A. **Schuster**, Ph.D., F.R.S.; G. **Johnstone Stoney**, M.A., F.R.S.

The following were elected Honorary Members:—

R. J. E. **Clausius**; H. L. **Fizeau**.

After the names had been announced from the Chair, Votes of Thanks were carried:—to the **President**; to the Lords of the Committee of Council on Education; to the **Demonstrator**, **Treasurer**, **Secretaries**, and to the **Auditors**.

THE TREASURER IN ACCOUNT WITH THE PHYSICAL SOCIETY, FROM DECEMBER 31ST, 1878, TO DECEMBER 31ST, 1879.

Dr.	£ s. d.	£ s. d.
Balance in Bank		55 9 6
" paid by Treasurer		4 15 2
Entrance-Fees		25 0 0
Subscriptions for 1876		4 0 0
" 1877		10 0 0
" 1878		23 0 0
" 1879		131 0 0
" 1880		4 0 0
Compositions		170 0 0
One year's Dividend on £400 4 per cent. Furness Debenture Stock, less Income Tax, 6*s*. 8*d*.		15 13 4
One year's Dividend on £460 5 per cent. Midland Preference Stock, less Income Tax, 9*s*. 8*d*.		22 10 4
Sale of Everett's C.G.S.:—		
Taylor and Francis		5 3 6
1 copy paid Treasurer		0 2 6
Donation from R. Sabine, Esq.		100 0 0
		£570 14 4

Cr.	£ s. d.	£ s. d.
Honorarium to Prof. Everett		20 0 0
Library:—		
Karl's Repertorium		12 0 0
Williams and Norgate		21 7 6
Reports of Meetings		13 7 3
Chapman:—		
Attendance	3 5 0	
Petty Cash	1 3 6	
		4 8 6
Messrs. Taylor and Francis:—		
Part v. vol. ii., Proceedings		37 2 6
Postage and addressing		3 10 6
Members' extra copies		10 1 0
Miscellaneous Printing		4 7 3
Part i. vol. iii., Proceedings		42 12 6
Postage and addressing		3 6 0
Members' separate copies		4 10 0
Miscellaneous printing		5 12 3
Part ii. vol. iii., Proceedings		37 4 6
Postage and addressing		3 3 6
Members' extra copies		6 5 6
Miscellaneous printing		4 12 0
Wheatstone Reprint:—		
Printing		137 5 6
Packing and Postage		6 19 0
Advertising		3 2 0
Index		5 0 0
Binding		16 9 1
De La Rue, Stationery		2 2 0
Richardson, printing		2 1 6
A. J. Clark, engraving		0 6 0
Cheque Book		0 2 6
Loss on Cheques		0 2 2
Petty Cash:—		
Dr. Atkinson	2 9 6	
Mr. Reinold	1 15 4	
Mr. Roberts	0 18 6	
		5 3 4
Balance in Bank		136 5 2
" due by Treasurer		22 5 4
		£570 14 4

Audited and found correct,

FRANK CRISP,
GEORGE S. CLARKE, } *Auditors.*

London, February 14, 1880.

PROPERTY ACCOUNT. PHYSICAL SOCIETY.

ASSETS.	£	s.	d.	LIABILITIES.	£	s.	d.
Subscriptions due	12	0	0	Subscriptions in advance	4	0	0
£400 4 per cent. Debenture Stock Furness Railway at 103	412	0	0	Balance	1143	10	6
£460 5 per cent. Preference Stock Midland Railway at 123	565	0	0				
Due by Treasurer	22	5	4				
Balance in Bank	136	5	2				
Stock in hand of Wheatstone's Papers and Proceedings of the Society							
	£1147	10	6		£1147	10	6

We have examined the above Account, and the Securities at the Bank, and find the same to be correct.

London, February 14th. 1880.

FRANK CRISP, } *Auditors.*
G. S. CLARK,